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BY WILLIAM NICHOLSON.

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PREFACE.

THE conclusion of the first Volume of the new Series of this Journal, demands that I should return my sincerest acknowledgments to those excellent Correspondents who have assisted me in improving it, and to the Public at large, who have shewn by the increased demand, that they approve the alterations which have been made.

Among the advantages I will here mention only two; the first by no means inconsiderable, is the very neat and correct typographical execution, which would be honorable to the Printer of a complete copy of a work at leisure, and is singularly valuable in a periodical work;—the other, namely the side notes, requires so much diligence and expence, that it is likely they will long continue to be peculiar to this Journal. These circumstances, added to the unvaried fidelity of conduct in quotation and regard to the rights of others; from which the Editor never has nor will depart, have naturally led men of eminence to chuse this Journal as the repository for their communications.

It is with a very high degree of satisfaction that I can state, that half the materials contained in the present Volume are original, and avowed by their very respectable Authors; that one third part consists of translations, or abstracts of excellent works never before published in English, and that the remaining sixth part are select Memoirs, either abridged or extracted from the Transactions of the Royal Society, and other recent and authentic sources of discovery.

The Authors of original Papers are Doctors Thomson, Beddoes, Higgins, and Priestley; the Rev. W. Pearson; Messrs. Gough, Close, Davy, Murray, R. L. Edgeworth, Stodart, Zach, Accum, Ez. Walker, Goodwyn, W. Walker, and W. N.—Of foreign works, Thenard, Peres, Tromsdorf, Prevost, Dolomieu, Proust, Loysel, Hauy, Volta, Van Marum, Le Blanc, Carradori, Darcet, Parmentier, Vauquelin, Darracq.—And of English Memoirs abridged or extracted, Mendoza, Herschel, Chenevix, Davy, Ware,

Cooper,

Cooper, Englefield, Delafons, Sheldrake, Capper, Arkwright. For the various and important objects of their research, he must refer to the Table of Contents, the ample Index, and the other assistant indications he has been sedulous to present to the Reader.

This Volume contains sixteen Copper Plates, illustrating nineteen distinct objects: viz. Herschel's discoveries of the Structure and Changes of the Sun. 2. Various circular Instruments for measuring Angles, 3, 4. Two Hydraulic Engines invented by Mr. Close for raising water above its Level by the Syphon. 5. Method of making Gun Flints. 6. Mr. Cooper's Designs to shew by the structure of the Ear, that a species of Deafneſs may be cured by puncture of the Membrane of the Tympanum. 7. Loysel's new Apparatus for bleaching Paper. 8. Carangeau's Instrument for measuring Crystals. 9. Dr. Thomson's Apparatus to shew that the Rumfordian Circulation of Solids in Fluids is not occasioned by Currents. 10. Volta's Sketches in support of his Theory of Galvanism. 11. Trevithack's new and powerful Engine for producing Mechanic Force by an included Column of Water. 12. Mr. Murray's Apparatus for new Experiments on the conducting Power of Fluids. 13. Dr. Herschel's Apparatus for viewing the Sun through Fluids. 14. Mr. Hornblower's Hydraulic Bellows for a Forge. 15. Rev. Wm. Pearſon's Projection of the Orbits of the Earth and of the new Planet Ceres, 16. Mr. Murray's Apparatus for shewing the conducting Power of Mercury and of Oil in a Veſſel of Ice. 17. A simple and free Eſcapement for Time Pieces, by Mr. Delafons. 18. Mr. Arkwright's Engine for raising Ores from Mines. And, 19. A cheap, simple, and accurate perspective Instrument, by Richard Lovell Edgeworth, Esq.

With prospects already ſo animating, under the favorable circumstances of a general Peace, and the facilities it affords to philosophical pursuits and manufacturing inventions, it is to be expected that the value of our communications and discoveries will increase.

Soho Square, London, May 1, 1802.

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APRIL 1802.

ERRATA.

I have obferved no Errata by which the Reader can be mifled, but muft remark, that the table at page 73, called *Obfervations* of the Ceres is wrong intitled: the table being merely an ephemeris, *calculated* for re-difcovering the planet from the former obfervations of Piazzi.

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JANUARY, 1802.

ARTICLE I.

On the Exhibition of a Series of Primes, and the Resolution of a Compound Number into all its Factors. By Mr. J. GOUGH.

To Mr. NICHOLSON.

SIR,

M. EULER remarks in his Algebra, that mathematicians have not yet discovered a way of exhibiting a natural series of primes; and in reality I know of no writer on arithmetic, who shews how a compound number may be resolved into all its factors, otherwise than by trial. The following method, however, may be said to solve both problems, perhaps in as ready a way as the nature of numbers will admit; and on fixed principles: it depends upon the following property of compound numbers. Natural series of primes and resolution of a number into its factors not yet effected.

If a compound number, which is not a square, be resolved into any two of its factors, one of these factors will be less than the root of the next greater square, and the other will be equal to, or greater than the same root.—If you deny the property, then both factors must be either greater than the root of the square, in which case the product will be greater than the square; or, they must be both less, than that root, on which Property of compound numbers.

ſuppoſition the product will not be greater than the next leſs ſquare, ſeeing the factors are whole numbers. Now ſuppoſe $f^2 - c$ to repreſent any number whatever, where f^2 is the next greater ſquare, and c the exceſs of that ſquare above the given number: then any two factors of $f^2 - c$ may be expreſſed by $f - p$, and $f + p + 2n$, from whence the following method of determining all the factors to two ſucceſſive ſquares, and their intermediate numbers, is eaſily derived.

Method of determining factors.

In a perpendicular column at the left hand of your future work, (*vide Tab.* 1ſt.) which exhibits the diviſors of all the numbers from 49 to 36, write down the arithmetic progreſſion 1, 2, 3, &c. beginning at the top with unity, and ending with f; let all theſe numbers ſucceſſively repreſent the diviſor $f - p$, and correſponding to each of them, draw a double horizontal column: when $f - p = 1$, make c in the uppermoſt horizontal column $= 2f - 1$, $3f - 2$, $2f - 3$, &c. to 0, becauſe $2f - 1$ is the difference of the two ſquares in all caſes; and for $f + p + 2n$, write $\overline{f-1}|^2$, $\overline{f-1}|^2 + 1$, $\overline{f-1}|^2 + 2$, &c. to f^2. In order to fill up the other columns, divide f^2 by the reſpective value of $f - p$, and place the remainder, if there be any, in the upper part of the horizontal column, perpendicularly under the ſame value of c in the higheſt column; but if there be no remainder, place a cypher in its room: then increaſe this remainder as often as you can by the addition of $f - p$, not to make it exceed $2f - 1$, and place theſe, each directly under the ſame number in the higheſt column, and write c on the left hand. Under the firſt remainder or cypher, place the quotient $\frac{f^2}{f-p}$ in whole numbers; from which, ſubtract unity as oft as $f - p$ was added to the remainder, and call theſe $f + p + 2n$; but when $f - p = f$, put 0 alone for the value of c: when all the columns are filled up in this manner, the factors of any given number are thus found. Find the value of c correſponding to that number in the higheſt column; then find how often the ſame number recurs, by caſting your eye perpendicularly down the columns, the factors will be the value of $f + p + 2n$, directly under that value of c in each column; and the correſponding value of $f - p$: conſequently, theſe values of c which are only found in the higheſt column, indicate that the number

number directly under it is a prime, as is the case in *Tab.* 1st, with 37, 41, 43, 47.

Primes.

But if a series of primes only is to be calculated, the work may be shortened, by omitting the values of $f+p+2n$ in all the columns except the highest, and marking those numbers with asterisks which correspond to the values of c only found in the uppermost line, as in *Tab.* 2d. where the prime numbers from 1 to 16 are calculated.

Note, the values of c may be otherwise calculated:—from the product fp take the greatest multiple of $f-p$ that can be subtracted from it; this will give the least value of c corresponding to that value of $f-p$.

Your's, &c.

Kendal, Dec. 7, 1801. J. GOUGH.

TABLE I.

$f^2 = 49$

$f-p=1$	$c=$	13	12	11	10	9	8	7	6	5	4	3	2	1	0
	$f+p+2n$	=36	37	38	39	40	41	42	43	44	45	46	47	48	49
$f-p=2$	$c=$	13		11		9		7		5		3		1	
	$f+p+2n$	18		19		20		21		22		23		24	
$f-p=3$	$c=$	13			10			7			4			1	
	$f+p+2n$	12			13			14			15			16	
$f-p=4$	$c=$	13				9				5				1	
	$f+p+2n$	9				10				11				12	
$f-p=5$	$c=$					9					4				
	$f+p+2n$					8					9				
$f-p=6$	$c=$	13						7						1	
	$f+p+2n$	6						7						8	
$f-p=f$	$c=$														0
	$f+p+2n$														7

TABLE II.

		From 1 to 4				From 5 to 9					From 10 to 16						
$f-p=1$	$c=$	3	2	1	0	4	3	2	1	0	6	5	4	3	2	1	0
	$f+p+2n$	*1	*2	*3	4	*5	6	*7	8	9	10	*11	12	*13	14	15	16
$f-p=2$	$c=$				0		3		1		6		4		2		0
$f-p=3$	$c=$									0			4			1	
$f-p=4$	$c=$																0

II.

On an improved Reflecting Circle. *By* JOSEPH DE MENDOZA RIOS, *Esq. F. R. S.* (*Phil. Transf.* 1801.)

Advantages of large inftruments in diminifhing the errors of divifion and eccentricity.

IN practical aftronomy large inftruments are ufeful, not only to enable the obferver to read the angles to a fmall fraction of a degree, but likewife to diminifh, in the conftruction, the inaccuracies which proceed both from the errors of the divifions and the eccentricity of the index. Frames of confiderable dimenfions admit alfo the application of telefcopes with great magnifying powers, which is a circumftance of the utmoft importance in celeftial obfervations. As the reflecting inftruments employed at fea are fupported by the hand, their weight and fcale are limited within a narrow compafs; and it feemed very difficult to obviate, by any expedient, the inconveniences arifing from the fmallnefs of their fize, while it was impoffible to increafe it. The celebrated Tobias Mayer contrived, however, a method to determine, at one reading, inftead of the fimple angle obferved, a multiple of the fame angle; and, by this means, the inftrument became, in practice, capable of any degree of accuracy, as far as regards the above mentioned errors. His invention is effentially different from the mere repetition of the obfervations; and my object requires that I fhould explain the principle upon which it is founded.

Invention of Mayer for diminifhing the fame errors at pleafure in fmall inftruments by reflection.

Mr.

Mr. Mayer propoſed to complete the limb of the ſextant, making a whole circle, with the horizon glaſs moveable round the centre, with an additional index, which I ſhall call *the horizon index*, in order to diſtinguiſh it from *the centre index*, to which the centre glaſs is attached. This inſtrument is repreſented in Plate I. Fig. 1; and the manner of uſing it is as follows: after the index A is ſet at o, (the beginning of the diviſions,) the two glaſſes are rendered parallel, as is uſually practiſed with Hadley's quadrant, by moving the horizon index B, till the horizon of the ſea, (or the ſun, or any other object,) or its direct image, and the doubly reflected image of the ſame, ſeen through the teleſcope, coincide. After fixing the horizon index in that poſition, the centre index A is to be moved, in order to meaſure the diſtance of the two objects S and L, (which I ſhall ſuppoſe the ſun and moon,) by bringing into contact the doubly reflected image of the ſun with the direct image of the moon, ſeen through the teleſcope. The centre image will then be at M, and the arch o M might give, as in the ſextant, the angular diſtance required; but the conſtruction of the circle renders it eaſy, in this poſition, to effect again the paralleliſm of the glaſſes, and to make another obſervation of the contact, in the like manner as from o; which operation will bring the centre index to N. The index will then give o N, or double the diſtance; and, as it muſt be divided by 2, in order to have the angle required, the errors of diviſion and eccentricity, which, together, I ſhall call the *error of the inſtrument*, will be likewiſe reduced to one half. It is obvious, that by ſucceſſive repetitions of the ſame proceſs, triple, quadruple, &c. the diſtance may be obtained, and the ſaid error further reduced, in the inverſe ratio of the multiplication of the diſtance, to any degree of approximation required.

1. He made the limb an entire circle. And 2. he fixed the horizon glaſs upon an index moveable on the ſame centre, but independant of that which carries the glaſs called the index glaſs.

Uſe. Adjuſt the glaſſes parallel by the horizon index.

Obſerve the angle by the firſt index and fix it.

Make the glaſſes again parallel by the horizon index.

Obſerve the angle as before.

Adjuſt for paralleliſm, and repeat the obſervation.

And as this proceſs may be continued at pleaſure, and the firſt index will paſs over the meaſure of the angle as many times as there are repetitions; Divide the whole ſpace by the number of repetitions, and the quotient will be the correct angle; the errors of the inſtrument being diminiſhed in the ſame ratio.

The method of rendering the glaſſes parallel, by means of the horizon of the ſea, is not accurate, on account of the indiſtinctneſs of the images; and, when the ſun is uſed for that purpoſe, the obſervation becomes fatiguing to the eye. The repetition of that obſervation, by one or the other method, remained therefore a conſiderable inconvenience attached to Mr. Mayer's circle. The author himſelf ſeems to have been of that opinion, as he propoſed to provide the inſtrument with a diagonal rule, fixed upon one of the indexes, ſo that the

other

Objection. It is inconvenient to make the adjuftments for parallelifm. Happy invention of Borda for removing it.

other index fhould touch it when the glaffes were parallel; but an adjuftment of this nature muft be fubject to great errors, and was never adopted in practice. The Chevalier de Borda, wifhing to remove that imperfection, had the happy idea of rendering the parallelifm of the glaffes unneceffary, by fubftituting the obfervation of the angular diftance of the two objects, to that of the coincidence of the images of the fame object. This conftitutes the fecond great improvement of the reflecting circle, which it is neceffary for me to explain, before I proceed to the account of my own inveftigations *.

By a flight alteration in the conftruction he renders it practicable to obferve the fame angle twice, by moving the index in contrary directions without fhifting the face of the inftrument;

In Borda's circle, the telefcope is fixed at fome diftance from the centre, and the horizon glafs is carried near the border of the inftrument, as in Plate I. Fig. 2. By this arrangement, the rays of light can arrive at the centre glafs, both from the heavenly bodies fituated to the right of the horizon index, as S′, and from thofe fituated to the left, as S. Thus, if the glaffes are parallel to one another, when the centre index is at o, it is obvious that there are two ways of making the obfervations. While the direct image of the moon L is feen through the telefcope, the angular diftance to the fun, if at S, may be meafured by moving the centre index to *m*, in order to produce the contact; or, if the fun is at S′, the fame operation may be performed, by ufing the contrary motion to *m′*. The firft kind of obfervation, the Chevalier de Borda calls *obfervation to the left*; and the fecond, *obfervation to the right*. Suppofe now, that (the horizon index being fixed in the fame pofition) the diftance from L to S is obferved to the left, by bringing into contact the doubly reflected image of S with the image of L, feen without reflection; let us then turn the inftrument round, keeping it in the fame plane, fo as to have the direct image of S through the telefcope, and thus make an obfervation of the fame diftance to the right; the pofition of the centre index being in the firft obfervation at *m*, and in the fecond obfervation at *m′*, it is clear, that if o is the point where the parallelifm of the glaffes takes place, o *m* is equal to o *m′*; and, that the arch *m m′*, determined by the two pofitions, will give double the diftance.

which gives the double diftance, and requires not any adjuftment for parallelifm.

* It does not belong to my prefent plan, to explain the effect of Borda's improvement in correcting the errors which arife from the want of parallelifm in the furfaces of the glaffes. This will be fully confidered in another Paper, where I intend to give an account of feveral inveftigations which I have made upon that particular fubject.

It

It will be more convenient to have the centre index at o, when the firft obfervation is made, in order to take the double diftance at one reading, after the fecond obfervation. For this purpofe, the firft part of the procefs may be inverted, by previoufly fixing the centre index at the beginning of the divifions, and moving the horizon index H towards o, inftead of moving the centre index A to *m*, or towards H. Convenient manipulation.

The laft kind of obfervation, in which the incident ray, which produces the firft image upon the centre glafs, may be conceived to run double the angular diftance, paffing in its way over the line of collimation, has been called, by the Chevalier de Borda, *the croffed obfervation.* This is denominated the *croffed obfervation.*

The fame procefs may be repeated, by fixing alternately one of the indexes, and moving the other, and continuing fucceffive fets of obfervations; each fet of two croffed obfervations, one to the right and another to the left. The angle given by the inftrument, will be equal to double the angular diftance multiplied by the number of fets obferved, or, in other terms, to the angular diftance multiplied by the number of obfervations, which are always fuppofed to be made by pairs; an odd obfervation being of no value in this manner of ufing the circle. It may be repeated at pleafure.

I have expreffed myfelf as if the obfervations could always be made by looking alternately at each object through the telefcope, in order to bring into contact the doubly reflected image of the other object. This is not the cafe in the obfervations of the diftances from the moon to the fun, or a ftar; it being then indifpenfable to compare, by reflection, the brighteft of the two heavenly bodies; but there is a very eafy method of obviating that inconvenience. After the contact of the images of S and L is obferved, with the telefcope directed to L, the pofition of the plane of the inftrument may be inverted, turning it round the axis of vifion O B L; the incident ray will then anfwer to the point S′, equally diftant from L as S, and the croffed obfervations will ftill give S S′, or double the diftance. And will not require a direct view of the brighter object, if the plane of the inftrument be reverfed at the fecond obfervation.

Whether a circle is ufed fimply, as Mayer propofed it, or according to Borda's method, its peculiar advantages chiefly depend on the multiplication of the diftance required. I have therefore turned my attention to the improvement of this principle; and, with that view, I have contrived the conftruction which I am going to defcribe.

In

The author's improvement confifts in applying an entire moveable circle, *called the flying nonius*, within or upon the divided circle. Each of thefe circles may be attached to either the center index or horizon index by the preffure of a clamp.

In the croffed obfervations made with Borda's circle, the indexes move alternately through an arch which in the divifions, is equal to double the diftance: for example, the centre index comes, in the firft croffed obfervation, from m to m'; in the third croffed obfervation, from m' to m'', &c. and the horizon index, in the fecond croffed obfervation, to h': in the fourth croffed obfervation, to h'', &c. and, by each of the two indexes may be found the fame multiple of the diftance required. Let us now place the Nonius in a circle moving round the centre, over, or adjacent to, the ufual limb which contains the divifions: it will eafily be conceived. that, by attaching that circle, which I fhall call the *Flying Nonius*, alternately to each of the indexes, it will ferve as Nonius for both; and that, after any number of obfervations, it will give the compound motion of the two indexes. Thus, after the firft obfervation, the Flying Nonius will, at each croffed obfervation, advance double the diftance over the divifions, while each feparate Nonius, fixed on the indexes, requires a fet of two obfervations, to produce the fame effect in Borda's circle.

View and defcription of the *improved reflecting circle*.

Plate I. (XXX. XXXI. and XXXII.) exhibits a perfpective view, (a plan, and a fection,) of the inftrument, which, for the fake of diftinction, I fhall call my *Improved Reflecting Circle*. (The laft Plate is particularly intended to fhew the compound handle, which I have adapted to the inftrument, in order to hold it with convenience and eafe in every pofition *). Thefe three

* The ufe of Mayer's circle, or of Borda's, as conftructed till now, with only one handle attached to the centre, is extremely inconvenient in feveral pofitions, and particularly when it muft be kept inverted downwards during the obfervation. For this reafon, I thought it of importance to contrive fuch a fupport as would enable the obferver to hold the inftrument with the fame eafe in every direction. This is effected by means of the compound handle, attached to the horizon index, by the brace V and fcrew X, (Plates XXX. XXXI. and XXXII.) which index turns round the centre with the handle. When Mr. Troughton began to conftruct this fort of inftrument, I recommended to him this improvement, which he has adapted to his reflecting circle.

I fhall obferve here, that Mr. Troughton's circles are not of the kind which I have endeavoured to improve. The fcheme of his conftruction may be faid to confift in completing the limb of a fextant

three reprefentations, in which the fame parts are marked with the fame letters, are fufficient to give an accurate idea of the arrangement of the whole, and make it unneceffary for me to enter now into a minute detail of the mechanifm of the apparatus. I therefore fhall content myfelf with adding here only what concerns the general ufe, and the peculiar properties, of this inftrument.

M is the divided limb of the circle, and N the Flying Nonius, to each of which the horizon index may be occafionally attached, by means of the clamps D, C; as well as the centre index, by means of the other clamps A, B. The peculiar property of the inftrument being that of giving double the diftance, I have thought proper to divide the circle into 360 degrees, and not into 720 according to the nature of the fextant. Thus, after a croffed obfervation, the reading of the Nonius will, without reduction, exhibit the meafure of the fimple diftance. I have likewife extended the nonius round the circumference, fo that, by the coincidence of two divifions, the number of degrees will appear on the limb, and that of the minutes and feconds on the flying nonius. The manner of making the obfervations with this inftrument is as follows:

The circle is divided into whole degrees, and the flying nonius has its divifions extended all round its circle.

Adapt the o of the flying nonius to 360° of the limb; and then faften the two clamps A, B, of the centre index * E E, by which the divifions will be kept in the fame relative fituations. Then, tnrn round the horizon index F F, and make an obfervation of the diftance to the right. The contact muft be adjufted by the fcrew G, the clamp C being faftened.

Method of obferving. Clamp the center index both to the limb and nonius (fet with o oppofite 360°). Obferve an angle to the right by moving the horizon index; which then clamp to the nonius. Difengage the center index from the nonius, and repeat the obfervation to the left: the flying nonius will give the diftance.

tant to the whole circumference, and making it capable thereby of performing Borda's croffed obfervations, with as many noniufes as may be attached to the centre glafs. But, Mr. Troughton's inftrument is deprived of the principle invented by Mayer, for obtaining at one reading a multiple of the diftance required; which is the great property of circles, and, in my opinion, the beft means of diminifhing their errors.

* Properly fpeaking, neither the centre nor the horizon indexes act, in this inftrument, as fuch, both of them being deprived of the nonius, which is transferred to the flying circle; but, for the fake of perfpicuity, I continue the ufe of thofe expreffions, in order to diftinguifh the plates or rules which carry the centre and the horizon glaffes.

Leave

Leave this clamp faſtened, and looſen the clamp A; thus turn the inſtrument, and make a croſſed obſervation to the left, adjuſting by means of the ſcrew H, after having faſtened the clamp D. At the end of this obſervation, the flying nonius will give the diſtance. Faſten now the clamp A, and looſen the clamps B and C, leaving the clamp D faſtened; then turn the inſtrument again, and make a croſſed obſervation to the right. At the end of this obſervation, the flying nonius will give double the diſtance. By ſucceſſively inverting the uſe of the clamps, this alternate proceſs may be continued *ad libitum;* and each croſſed obſervation will increaſe the reading, by an arch equal to the diſtance.

Remarks. Let the number of obſervations be n, and the angular diſtance D. The arch given by my improved circle will be $= D(n-1)$. In Borda's circle, (reducing the diviſions of the ſextant to thoſe of the theodolite I uſe,) the arch is $= D \times \frac{1}{2} n$; and, either n muſt be an even number, or the odd obſervation muſt be loſt. In Mayer's circle, the arch is $= \frac{1}{2} D \times \frac{1}{2} n$; and the number n, which comprehends the obſervations for the paralleliſm of the glaſſes and thoſe for the diſtance, muſt likewiſe be even. The compariſon of theſe expreſſions, ſhews at once the relative advantages of the different inſtruments.

My conſtruction offers conſiderable advantages, in every manner of uſing the circle. If, inſtead of the croſſed obſervations, it ſhould be wanted to employ the uſual practice of rendering the glaſſes parallel, a multiple of the diſtance may ſtill be obtained by my inſtrument, equal to that of the other method. For this purpoſe, the paralleliſm of the glaſſes may be effected, by means of the images of the ſun, or the horizon of the ſea, moving the index F, while the o of the nonius is adapted to 360° of the limb, and the two clamps A, B, are faſtened. After this, an obſervation of the diſtance to the right may be made, with the clamp A faſtened, while the clamp B is looſe; the clamp D being alſo faſtened, and the clamp C looſe; and, at the end of this obſervation, the flying nonius will give an angle, which will be only the half of the diſtance in my diviſions, but which would be equal to the whole diſtance, if the diviſions were according to the ſextant. After that, and while the clamps B and C are faſtened, and the clamps A and D looſe, the paralleliſm of the glaſſes may be

be again effected; and the nonius will advance the same quantity over the limb. The same addition will take place, by inverting the use of the clamps, and making another observation of the distance. The like alternate process may be continued indefinitely; and the result given by the instrument will, with only one observation more, be the same as that of Borda's method, and double the arch which would be obtained by Mayer's circle.

Mr. Borda's circle is liable to a very great inconvenience in practice. Each index advances successively over the limb; and, in order to facilitate the operation of bringing the images for the contact within the telescope, that author advises to make a preparatory memorandum of the positions which the indexes will nearly occupy, so that they may be set accordingly, previous to each observation. But this method, which is always inconvenient, by night becomes almost impossible. For this reason, I have joined to the horizon index an arch L L, (Plates XXX. and XXXI.) which is divided, both to the right and left, into degrees and minutes of the sextant, so that, when the glasses are parallel, the centre index coincides with the two first divisions o, o, and occupies the space left blank between them. I have further provided two sliding pieces P, P, which may be adapted to that arch, with a spring sufficient to keep them firm in any situation. Putting each of these pieces upon the arch, so that their ends may coincide with the divisions marking the rough distance to be measured, no more will be required, than to set the centre index alternately against each piece, before the beginning of the successive crossed observation. The clamp may then be fastened, and the remainder of the motion produced by the adjusting screw; as, if necessary, the index will push the sliding piece further, and leave it at the point where the contact was effected *.

Easy contrivance for setting to the distance nearly, before each observation.

The flying circle facilitates the use of any number of noniuses, which may be applied round the whole circumference; but, as the leading principle which I have chiefly had in contemplation, is that of obtaining an accurate result from one reading, I have only used a single nonius. Two noniuses,

Other advantages, such as the increased number of noniuses, if required.

* The idea of this simple contrivance, was suggested to me by the ingenious Mr. E. Troughton.

opposite

oppofite one another, might however be advantageous, in order to correct the errors of eccentricity; but, in my opinion, a greater number ought not, in any cafe, to be ufed.

It may be ufed in circles where the telefcope is attached.

Before I conclude this Paper, I fhall remark, that my improvement may be partially applied to a circle, where the telefcope and the horizon glafs are attached, or fixed, to the main frame of the inftrument. The flying nonius, acting then with the fingle centre index, will only give the fame refult as Borda's circle; but this conftruction feems to me greatly preferable to all the other plans executed till now; the whole apparatus being more folid and fimple, and its ufe not liable to the errors which arife from the motion of the horizon index.

It affords a method of repeating obfervations beginning from a ftop.

With this conftruction, we may likewife employ a method of afcertaining the place where the parallelifm of the glaffes was obferved to take place, and of fetting the index afterwards in the fame fituation, as often as is neceffary for the repetition of the obfervations. A piece may be ufed, fo contrived as to be attached to, or detached from, one fide of the index, by means of a fcrew; and provided befides with other fcrews, to faften it to any part of the limb. This rectification piece, being previoufly attached to, and carried with, the index, muft be fixed in the fituation it occupied when the contact of the images was obferved. The index will then be detached from it, in order to obferve the diftance, and afterwards muft be brought back to the fame pofition as before, contiguous to the rectification piece. The like alternate procefs may be repeated; and the flying nonius, going with the index in the motions forwards, and ftanding ftill in the motions backwards, will give the multiple of the obferved angle, without performing the obfervation for the parallelifm more than once in the beginning. In Mayer's circle, as well as in Borda's, there is a great objection to any attempt for that purpofe; becaufe, as the horizon glafs moves round with the index, its perpendicular pofition is deranged by the inequalities of the plane of the limb; but, in my conftruction for multiplying with the horizon glafs fixed, that inconvenience is removed; and the method of afcertaining the identical pofition of the glaffes may be employed in practice, with advantage, it being done when the index is at the fame point of the frame. By fuggefting this idea, I do not, however, mean to reprefent it

it as preferable to the repetition of the obfervations; which procefs muft, for many reafons, have the advantage over any mechanical contrivance whatfoever.

I have procured reflecting circles to be conftructed, upon the principles here defcribed, both with the telefcope and the horizon glafs upon a moveable index, and with the fame pieces attached to the main frame of the inftrument. The two methods have refpectively anfwered my expectation; and I purpofe, at a future opportunity, to publifh a defcription of the means which I wifh to recommend for the mechanical improvement of the different parts, together with an account of fome other effays which I have made relative to the fame fubject.

The invention fucceeds in practice.

III.

Obfervations tending to inveftigate the Nature of the Sun, in order to find the Caufes or Symptoms of its variable Emiffion of Light and Heat; with Remarks on the Ufe that may poffibly be drawn from Solar Obfervations. By WILLIAM HERSCHEL, LL.D. F.R.S.*

ON a former occafion this eminent aftronomer gave his reafons for inferring that the fun is a moft magnificent habitable globe In the prefent paper he proceeds confiderably farther in the inveftigation of the phyfical and planetary conftruction of that luminary. He introduces his fubject by remarking, that the influence of this luminous body on the globe we inhabit is fo great and fo widely diffufed, that it becomes almoft a duty for us to ftudy the operations which are carried on at its furface; that from motives arifing from the important effects of light and heat we ought certainly to examine their fource, and though, like the Egyptians with the Nilometer, or modern philofophers in their obfervations upon the atmofpheric phenomena, we can entertain no hope of modifying the effective caufes, yet, by arranging the particulars of our own conduct by the fore-knowledge of events though inevitable, we may add much to the general felicity and enjoyments of fociety.

The fun inferred to be an habitable globe.

Its influence of the greateft importance to us,

and deferves to be ftudied.

* Abridged from the Philofophical Tranfactions, 1801, p. 265.

New terms of solar phenomena.

Dr. Herſchel, in conſequence of the improvements in his teleſcopes, and the advances in his knowledge of the phyſical conſtruction of the ſun, has rejected the old terms of ſpots, *nuclei, penumbræ & luculi,* and has ſubſtituted thoſe of openings, ſhallows, ridges, nodules, corrugations, indentations, and pores.

The luminous matter of the ſun is of the nature of clouds, or atmoſpherical phenomena.

In his former paper on the nature and conſtruction of the ſun and fixed ſtars, the author ſhewed that the lucid ſubſtance of the ſun is not a liquid nor an elaſtic fluid, but that it exiſts in the manner of luminous clouds ſwimming in the tranſparent atmoſphere of the ſun, or rather of lucid decompoſitions taking place within that atmoſphere.* His preſent obſervations confirm and extend that induction. They are arranged under certain enunciations, which I ſhall proceed to give, with ſo much of the particular facts as may be requiſite for their elucidation.

Of Openings.

Openings; or places where the luminous ſolar clouds are removed.

Particular deſcription.

Openings are places where the luminous clouds of the ſun are removed, ſo as to exhibit the opaque globe of the ſun through the aperture. On the 4th of January, 1801, there was a large opening much paſt the centre of the ſun, with a ſhallow about it, as exhibited in Fig. 1, Plate II. On the preceding ſide the thickneſs of the ſhallow was viſible from its ſurface downwards, but on the following ſide the edge of the ſhallow only could be ſeen, but not its thickneſs. The ſide of the elevation ſurrounding the ſhallow was alſo ſeen going curvedly down to the ſurface of the ſhallow on its preceding ſide. Fig. 2 repreſents a ſection of the ſame opening, in which the lines *a b c d f* are ſuppoſed to be drawn from the eye of the obſerver, correſponding with lines marked by the ſame letters in Fig. 1. The line *d* goes through the opening to the ſurface of the ſun A B. Fig. 2 ſhews evidently why the thickneſs of the ſhallow and elevation of the luminous parts are ſeen on one ſide and not on the other.

General facts reſpecting the ſolar openings.

Large openings have generally ſhallows about them. Many openings are without ſhallows. Small openings are generally without ſhallows. Openings have generally ridges and nodules about them. The openings have a tendency to run into each

* Philoſ. Tranſ. 1795, p. 72.

other,

other, and new ones break out near those already formed. The probable cause of openings is, that an elastic, but not luminous gas, issues up through the pores or incipient openings, and spreads itself on the luminous clouds, forcing them out of the way, and widening its passage. The direction and operation of this cause appears to be not equally extended on all sides, but is frequently oblique, so that the luminous clouds are driven, and form a larger shallow on one side. Different openings being effected similarly, seem to shew a correspondent effect or unity of direction to some extent.

They are produced by a wind or gas from the sun's body.

Fig. 3 represents an opening, with a branch coming from its shallow, seen February 18, 1801. Fig. 4 represents the same opening as it appeared one hour afterwards, broken out at three places on the one side, with corresponding projections in its shallow, which is very large on the side towards which those breakings out point, but is more confined on the other quiescent side.

Changes in the openings observed.

Fig. 5 represents a small oblong opening, with a very long shallow, the luminous matter being close to the opening on the other sides. Eight other small openings, forming a cluster, had each their shallows on the same side. In three hours it changed to the appearance at Fig. 6, and an hour afterwards an opening began to appear in the furthest end of the shadow, as at Fig. 7. When the shallows begin to diminish, and the lips or projections to disappear, the openings were observed to be at the period of their greatest extent. Fig. 8. There is some difference in the colour of openings, apparently from a thin veil of luminous clouds hovering above them. When the openings are decaying, they divide as in Fig. 9, in which observation, December 27, 1799, the luminous passage across the opening resembles a bridge thrown over a cave or hollow space. The opening was completely gone the following day. Openings sometimes increase after beginning to diminish. They generally grow less and vanish. They sometimes become converted into large indentations, with or without pores or small openings, and when they have vanished, they leave the surface disturbed more than common.

Observations.

The depth of the openings is visibly apparent, and the distance between the shallows and the solar surface is indicated by the free motion of low clouds. Fig. 10 shows a large

Observation of the depth, &c. of openings: Low moving clouds.

large opening obferved January 25, 1801, at 9^h 22^m. The note is as follows: "A large opening, which I have been obferving fince the 19th, is now much advanced towards the limb. I can fee into it; and on the preceding fide, as it appears to me, a good way under the loweft regions of the clouds of which the fhallow confifts. The upper margin of the fhallow is very fharply determined; but the clouds at the lower part of it, on the contrary, are more difperfed; fome of them hanging a good way down towards the furface of the fun's body. See Fig. 11. At 10^h 20^m the preceding fide of the fhallow of the large opening is now more abruptly terminated at the bottom of its thicknefs; the hanging or projecting clouds being removed towards the following fide. See Fig. 12."

Narrative.

Of Shallows.

Shallows, or places depreffed below the luminous clouds.

Shallows are depreffed below the general furface of the fun, and are places where the luminous folar clouds of the upper regions are removed. Their thicknefs is vifible. They fometimes exift without openings in them. They begin from the openings or branch out from fhallows already formed, and go forwards. The probable caufe of fhallows is deduced from the obfervations, among which the following are delineated.

Obfervations of the fhallows;

January 25, 1801, 9^h 20^m. Two branches, A B, Fig. 13, of a fhallow coming from an opening C, going towards the fouth. It feems as if they were deftined to meet the incipient fhallow of a fouth-following opening D. 9^h 50^m the fhallow B is very nearly united to the narrow part of the fhallow furrounding the opening D. The fhallow H feems to advance in a direction towards the fartheft fouth-following opening E. 10^h 20^m the fhallow B is completely run into the fhallow about D; and the fhallow A is grown broader towards F. 11^h 30^m the fhallow B is fo completely joined to the fhallow D, tbat it appears as if it had not come from the opening C. The fhallow A now ends in a fharp point, Fig. 15. 12^h 50^m the fhape of the fhallow A is no longer pointed, but very broad at the end, and there is a new branch breaking out at G, Fig. 16. Thefe changes feem all to denote that the fhallows are occafioned by fomething coming out of the openings which, by its propelling motion, drives away the luminous

they are caufed by the propelling gas which produces the openings.

luminous clouds from the place where it meets with the leaſt reſiſtance; or which by its nature diſſolves them as it comes up to them. If it be an elaſtic gas, its levity muſt be ſuch as to make it aſcend through the inferior region of the ſolar clouds, and diffuſe itſelf among the ſuperior luminous matter. 1^h 10^m the new branch G increaſes, and the openings C D E are enlarged. A new branch is alſo breaking out from the ſhallow about E. It is marked H in Fig. 14, and denoted with points. Theſe changes ſeem to prove that the ſame gas which diffuſes itſelf over the ſhallows, has forced open the paſſages at firſt, and is now widening them. Hence the increaſe of the openings is an additional circumſtance which points out the cauſe of the ſhallows. 1^h 20^m: from the ſhallow of a very large preceding opening, which is in an increaſing ſtate, are lately projected three ſmall branches, *a b c*, Fig. 17. 2^h 30^m the vacancies between the three ſmall projecting ſhallows are now filled up by the ſame cauſe that occaſioned them, ſo as to have given them the ſhape of an uniform but broader ſhallow on the ſide where the points came out.

Particular appearance of the ſhallows. They are tufted like maſſes of denſe clouds.

The ſhallows have none of the appearance called corrugations, but are tufted. The cloſe connection of the tufts makes them appear as if in every vacancy there were clouds under clouds, that prevented our looking far into them. The decay of the ſhallows appears to be produced by the encroachment of the luminous clouds on all ſides, in conſequence of the diminution of the energy which cauſed them."

Of Ridges.

Ridges, or elevations of the luminous clouds of the ſun.

Ridges are elevations above the general ſurface of the luminous clouds of the ſun. The length of one of the longeſt was found to be about 75,000 miles. They generally ſurround the openings, though they are often ſeen in places where there are no openings. They are ſoon diſperſed. From the appearances, the author thinks it probable that the luminous matter is diſturbed at top by the tranſparent elaſtic fluid which iſſues from the openings, or that it may act below the luminous clouds, ſo as to lift them up or increaſe their maſs.

Of Nodules.

Nodules, or ſmall elevations of the luminous matter.

Nodules are ſmall, but highly elevated luminous places. They may be ridges four ſhortened.

Of Corrugations.

Corrugations are ſmaller elevations and depreſſions of the luminous matter.

The corrugations conſiſt of elevations and depreſſions. They have a mottled appearance, conſiſting of dark and bright places. Many of the dark places are not round, but a little extended in different directions, and appear to be lower than the bright places. On favourable days the corrugated ſurface preſents its elevations and depreſſions as diſtinctly as the rough ſurface of the moon. They extend over the whole ſurface of the ſun. Diſperſed ridges or nodules make corrugations. The corrugations change their ſhape and ſituation; they increaſe, diminiſh, divide, and vaniſh quickly. Fig. 18 exhibits a ſmall ſketch of the place of phenomena obſerved by the author and Dr. Wilſon, in which the rapid changes were correſpondently obſerved.

Of Indentations.

Indentations of the luminous matter.

The dark places of corrugations are indentations. That they are not much depreſſed, is deduced from their viſibility pretty near the margin of the ſun. The indentations go down at the ſides like circular arches, but their bottoms are occaſionally flat. See Fig. 19. Some indentations have no openings, others have. The corrugations are of all ſhapes, chiefly lengthened. The indentations are of the ſame nature as ſhallows; are of different ſizes; ſometimes containing very ſmall openings, and ſometimes changing to openings. They are extended over the whole ſurface of the ſun; and with low magnifying powers they appear like points.

Of Pores.

Pores.

The low places of indentation are pores. They occaſionally increaſe, and become openings: and frequently they vaniſh in a ſhort time.

Of the Regions of Solar Clouds.

The ſhining matter of the ſun cannot be a liquid or a gas;

Dr. Herſchel obſerves that the phenomena before deſcribed could not appear, if the ſhining matter ot the ſun were a liquid, becauſe by the laws of hydroſtatics the openings, ſhallows,

ſhallows, indentations, and pores would be filled up, and the ridges and the nodules would ſubſide almoſt inſtantly. Whereas many openings have been known to laſt for a whole revolution of the ſun, and extenſive elevations have remained ſupported for ſeveral days. Still leſs could theſe phenomena conſiſt with the ſuppoſition of elaſtic fluidity. It remains, therefore, ſays he, for us to admit this ſhining matter to exiſt in the manner of empyreal luminous or phoſphoric clouds, reſiding in the higher regions of the ſolar atmoſphere. In ſupport of this notion he gives the following obſervations.

And conſequently it muſt form clouds.

Rapid ſketch of the phenomena of the ſun's ſurface.

Changes in the ſolar clouds happen continually. There are two different regions of ſolar clouds, the lower of which conſiſts of clouds leſs bright than thoſe which compoſe the upper ſtratum. The inferior clouds are opaque, the colour of all the ſhallows being the ſame, and conſequently not affected by the cauſe which acts upon the upper ſtratum when ſhallows are generated. By a contrivance for the uſe of his photometer, he found that if the ſuperior ſelf-luminous clouds of the ſun throw the ſame quantity of light on the inferior region of opaque ſolar clouds as they ſend to us, thoſe inferior clouds, of which the ſhallows are compoſed, reflect about 469 rays out of 1000 they receive; but that the ſolid ſurface of the ſun ſeen in the openings reflects no more than about 7. The indentations are planetary clouds, reflecting light through the open parts of the corrugations, and thoſe opaque inferior clouds probably ſuffer but little of the light of the ſelf-luminous ſuperior clouds to ſtrike the body of the ſun. The motion of the inferior clouds is ſeen through the openings as they paſs along. The ſuperior clouds are alſo ſeen to tranverſe the ſame apertures previous to their cloſing.

Luminous clouds.

Dark or planetary clouds.

Other facts and appearances.

Uſe and conſtitution of the ſolar clouds.

That the planetary clouds are of eminent uſe, is inferred by recurring to the phenomena before recited. The planetary atmoſphere of the ſun, its great height, its denſity, as inferred from the power of gravitation, which is known to be twenty-ſeven times ſtronger at the ſun's ſurface than with us, and muſt accordingly condenſe the atmoſpheric gaſes, its agitation ſimilar to the winds of our planet, and the clear atmoſpheric ſpace beneath the ſhallows, are among the facts upon which our philoſopher extends his diſcuſſions. He repeats and amplifies his remarks reſpecting the operation of the gas, which is ſtated to paſs from the ſun itſelf upwards to the region of

 the

the clouds, fo as to generate pores, corrugations, and all the folar changes, and moft probably to maintain phenomena in the fun's atmofphere, which in ours would be mere tranfitory corrufcations, like thofe of the aurora borealis, but are there, on account of the greater denfity, fo compreffed as to become much more efficacious and permanent. If this account of the folar appearances fhall be well founded, he concludes that there will be no difficulty in afcertaining the actual ftate of the fun with regard to its energy in giving light and heat; and that nothing will now remain but to decide the queftion which will naturally occur, whether there be actually any confiderable difference in the quantity of light and heat emitted from the fun at different times. To afcertain this he has recourfe to his journal.

Inquiry whether the energy of the folar heat and light be dependant on thefe phenomena?

Journal of folar obfervations.

In the firft place he gives a fet of obfervations, in which the figns of fcarcity of luminous matter in the fun were apparent; namely, in general a deficiency of the luminous or empyreal clouds, no ridges, nodules, corrugations, or openings. This period lafted from the year 1795 to 1800. A fecond fet, commencing in the year 1800, affords indications of a contrary nature. He expreffes his opinion that the character of the feafons may be greatly dependent upon thefe phenomena. But, in order to afcertain by fome correfpondent event or confequence that the feafons have been moft productive when the folar fpots were abundant, he recurs to the aftronomy of Lalande for the folar appearances, and to the table in Adam Smith's Wealth of Nations for the prices of wheat during the fame periods. The refults are, upon the whole, favourable to his theory; though every cautious inveftigator into the laws of the univerfe muft fee that it is greatly in want of the fupport of obfervation in both refpects. The Doctor, with his ufual candour, ftates fome of the difficulties of his fubject, and concludes by adding, that his prediction of plentiful harvefts from the prefent thriving ftate of the folar conftitution ought not to be relied on with more confidence, than the facts and arguments he has prefented may entitle it to. And from motives of the fame kind, I have alfo taken the liberty to condenfe this part of his paper rather more than the other.

Opinion that the feafons are governed by the variation in the fun.

Recurrence to Lalande for folar facts, and to Adam Smith for the fertility of the feafons.

IN

Subfequent obfervations.

IN a fubfequent paper of the fame volume, Dr. Herfchel gives a number of additional obfervations, made in the fpring of 1801, by which it appears that the fun continues to exhibit fymptoms of a plentiful emiffion of light. He ftrongly fufpects that one half of our fun is lefs luminous than the other, and that its variable luftre may poffibly appear to other folar fyftems as irregular periodical ftars are feen by us; but whether this arifes from fome permanent conftruction of the folar furface, or be merely an accidental circumftance, muft be left to future inveftigation.

Sufpicion that the fun has a bright and an obfcure fide.

He takes notice that we need not in future be at a lofs with regard to the current temperature of this climate, as the thermometrical obfervations, regularly given in the Philofophical Tranfactions, afford a proper ftandard of comparifon with the folar phenomena.

Judgment of the feafons by thermometrical regifters.

In this paper alfo, with a drawing, is defcribed a method of fubftituting liquids inftead of dark glaffes, for moderating the folar light and heat in telefcopes. It is a fquare trough, clofed at two oppofite ends by well polifhed parallel plane glaffes. On one fide of this trough is a fmall handle, and on the other a lip or fpout for pouring out any portion of the liquid when the reft is to be diluted. The trough is made to fit into an excavation in the eye tube of the telefcope, and in this fituation the folar rays muft pafs through the liquid therein contained before they arrive at the eye. By colouring the fluid, the obferver has it in his power to foften the light at pleafure; and water keeps off the heat fo as to prevent the leaft inconvenience. It is eafy to make trial of a variety of fluids in this way. Among other mixtures, the Doctor found that ink, diluted with water, and filtered through paper, fhewed the folar image as white as fnow, and very diftinct. Through this mixture he could obferve the fun in the meridian for any length of time without danger to the eye, or to the glaffes, with a mirror of nine inches in diameter, and the eye-pieces open, as they are ufed for night obfervations.

Very advantageous ufe of liquids inftead of the dark glaffes of telefcopes.

Apparatus.

IV.—Ob-

IV.

Obſervations and Experiments upon Dr. James's Powder, with a Method of preparing, in the humid Way, a ſimilar Subſtance. *By* RICHARD CHENEVIX, ESQ. F.R.S. M.R.I.A.*

Preliminary remarks.

AFTER the obſervations and experiments made by Dr. Pearſon, to inveſtigate the nature of Dr. James's powder, and preſented by him to this Society, very little remained to be effected or deſired, towards a further knowledge of the ſubject. But thoſe very experiments ſerved to ſuggeſt, that the mode of preparation was far from being the beſt that the preſent improved ſtate of chymical knowledge might afford; and that, in all probability, a leſs defective compoſition might reſult from a proceſs more conformable to ſome improvements, which of late have been advantageouſly applied to pharmaceutic chymiſtry.

It may be laid down as a general principle, that, in delicate experiments, whether analytical or ſynthetical, fire (that potent and once believed to be univerſal agent) is too precarious in its means, and too uncertain in its application, to be employed with full and conſtant ſucceſs. And, if it is ſtill recurred to, the advantage of promptneſs, and a remnant of ancient cuſtom, are the principal reaſons. But, where other methods can be deviſed to effect the ſame combinations, (and the humid way offers many,) every perſon converſant in chymical knowledge will allow the benefit of adopting them. The recent improvement in the mode of preparing calomel, is a ſtriking example of ſuch ſalutary corrections being ſucceſsfully introduced.

The method of preparing James's powder, or *Pulvis Antimonialis.*

A few obſervations upon the formula according to which Dr. James's powder, or the *Pulvis Antimonialis*, is prepared, and upon ſome properties of antimony, will place this aſſertion in a more prominent point of view.

In order to prepare this powder, we are told to take equal weights of bone or hartſhorn ſhavings and crude antimony, and calcine them together, at a high temperature: in other words, to take phoſphate of lime, which already contains a great exceſs of lime, and add to it an oxide of antimony. In

* Philoſ. Tranſ. 1801.

this

this procefs, it has been fuppofed, that the phofphoric acid of the bone or hartfhorn will faturate, not only the lime with which it was originally combined, but, in addition to it, a new portion of metallic oxide, and a new portion of lime. For, what little fulphuric acid might, during the procefs, have been formed by the combuftion of the fulphur of the crude antimony, is diffipated, at a much lower temperature than that to which the powder is expofed.

Every oxide of antimony with which we are acquainted, is volatile at a high degree of heat: it would therefore be hazardous to affert, that it is poffible to preferve always the fame proportion of antimony, whatever care may be employed in directing the operation; and, a diffimilarity in the chemical refult, muft neceffarily be attended with uncertainty in the medical application. It contains an uncertain dofe of antimony;

To this property may be added another, no lefs conducive to error. That portion of oxide of antimony which is not volatilized, becomes, in a great meafure, infoluble in all the acids. What the effect of the gaftric juice may be, upon a fubftance which refifts the action even of nitro-muriatic acid, it is not my purpofe to determine. It is fufficient for me to fay, that, as the quantity of infoluble matter, in a given quantity of Dr. James's powder, prepared at different times, may vary, the effect of any dofe alfo may differ, according to the proportions of foluble and infoluble matter. and the oxide varies in its folubility.

I look upon it as a fortunate circumftance, that thofe experiments and obfervations which I mentioned in the beginning of this paper, exifted as a ftandard to which I might refer my own attempts, and by which I might eftimate their validity. Dr. Pearfon has proved, (as by my own experiments I have found,) that in Dr. James's powder about 28 per cent. refifted the action of every acid. In examining fome of the *Pulvis Antimonialis* of the London Pharmacopeia, I found the average quantity of infoluble matter to be about 44 per cent. This proportion, however, was liable to confiderable variation.* Facts in proof.

* I find, from the information of feveral medical gentlemen, that the *Pulvis Antimonialis* is generally confidered as ftronger than Dr. James's Powder. This feems rather extraordinary, when we confider that the quantity of infoluble matter is greater in the former than in the latter; and would almoft lead us to fufpect it to be the active part of the medicine.

The

Whether this powder be a ternary compound.

The powder here treated of is denominated, by Dr. Pearſon, a triple ſalt, or a real ternary combination of a double baſis, (lime and antimony,) with phoſphoric acid. What I have mentioned, with regard to the quantity of acid contained in bone or hartſhorn, as being too ſmall to ſaturate a new portion of theſe baſes, may throw ſome doubts upon the poſſibility of any ſuch combination in the preſent caſe. But I have made ſome more direct experiments, which tend to prove, that no ſuch combination does exiſt.

It contains no phoſphate;

I took ſome white oxide of antimony, (formerly called Algaroth Powder,) precipitated by water from muriate of antimony, and heated it for a long time with phoſphoric acid. I decanted the liquor, and waſhed the powder that remained. No antimony could be found in the liquor; nor could any traces of phoſphoric acid be detected in the reſiduary oxide of antimony. I then took a ſolution of muriate of antimony, and divided it into two equal parts: into one, I poured diſtilled water; and, into the other, a ſolution of phoſphate of ſoda. In each liquor, a copious precipitate was formed; which precipitates, after being well waſhed, were dried. The weight of both was the ſame; whereas, it is evident that, had any phoſphoric acid been combined with the oxide, there would have been an augmentation of weight, in that which was precipitated by the ſolution of phoſphate of ſoda. This precipitate likewiſe, upon examination, gave no traces of phoſphoric acid. From theſe experiments it appears, that there exiſts no combination, which can be denominated a phoſphate of antimony.

neither does it appear to be antimoniate of lime.

To attempt an explanation of the real nature of the powder here ſpoken of, I had recourſe to ſome experiments of Monſ. Berthollet. By detonating ſulphuret of antimony and nitrate of potaſh, in a crucible, he obtained a maſs, whieh he reduced to powder, and waſhed. The liquor gave, upon evaporation, a cryſtallized ſalt, which M. Berthollet terms an *antimoniate of potaſh*. I never could ſucceed in any attempt to form a ſimilar combination between the above white oxide of antimony and potaſh, owing, I believe, to the ſmall quantity of oxigen contained therein, compared with that which is combined wlth the oxide obtained by detonation. I cannot therefore ſay, that the powder in queſtion is, in any degree, what M. Berthollet would call an *antimoniate of lime*.

But,

But, be the ſtate, whether of mixture or of combination, what it may, my purpoſe is to endeavour to produce a ſubſtance, which, from its more certain mode of preparation, may be more equal and conſtant in its effects.

New proceſs for a combination of antimony of more conſtant effect.

Diſſolve, together or ſeparately, in the leaſt poſſible portion of muriatic acid, equal parts of the forementioned white oxide of antimony and of phoſphate of lime.* Pour this ſolution gradually into diſtilled water, previouſly alkalizated by a ſufficient quantity of ammonia. A white and abundant precipitate will take place, which, well waſhed and dried, is the ſubſtitute I propoſe for Dr. James's powder.

Remarks.

The theory of this precipitation is ſo clear and ſimple, that it does not require any comment. It may be uſeful, however, to thoſe who wiſh to make this preparation, to remark, that it is abſolutely neceſſary that the ſolution of phoſphate of lime and of oxide of antimony, in muriatic acid, ſhould, after being well mixed, be poured *into the alkaline liquor*, in order to obtain a precipitate homogeneous throughout the operation. For, ſhould the alkaline liquor be poured *into the acid ſolution*, the water of the former would act upon the entire maſs of oxide of antimony, while the alkali would precipitate the phoſphate of lime only as it ſaturated the acid which held that ſalt in ſolution: thus, the precipitate would contain more antimony in the beginning; and, towards the end, the phoſphate of lime would be predominant. For the ſame reaſon too, a pure alkali is preferable to its carbonate; for the carbonic acid diſengaged, would retain in ſolution a portion of phoſphate of lime.

* In order to procure the phoſphate of lime, I diſſolved in muriatic acid, a quantity of calcined bone, and precipitated by ammonia, in its ſtate of greateſt cauſticity. By this means, the exceſs of muriatic acid, which held in ſolution the phoſphate of lime, is ſaturated, and the phoſphate is precipitated; but no muriate of lime is decompoſed, if the ammonia is quite free from carbonic acid. This is the moſt direct method of obtaining phoſphate of lime pure. This ſalt is not decompoſed, as ſome have aſſerted, by muriatic acid, but merely diſſolved by it. I have been induced to ſtate fully theſe particulars, becauſe, from the beneficial effects of this ſalt in the treatment of rachitis, as propoſed by M. Bonhomme, (*Annales de Chimie*, Vol. XVIII. p. 113,) it may become of general uſe. The oxide of antimony, I obtained by precipitating, by water, the common butter of antimony of the ſhops.

2 Whether

It is probably a mixture.

Whether this compofition be a chymical combination or a mixture, I will not take upon me to determine; but, for the reafons above mentioned, in fpeaking of Dr. James's powder, I believe it to be merely a very intimate mixture. At all events, it muft be more homogeneous than any that can be prepared in the dry way. It is entirely foluble in every acid that can diffolve either phofphate of lime or oxide of antimony feparately; and, to have it conftantly and uniformly the fame, no further addrefs, in preparing it, is required, than to avoid the errors I have mentioned.

Stronger preparation.

As, after fome medical trials of the powder, it was fuggefted to me, that it might be advantageous to render it fomewhat ftronger, I prepared another portion, by taking two parts of oxide of antimony and but one of phofphate of lime, and precipitating as above defcribed. The medicinal power was then confiderably increafed.

Medical teftimony of its ufe.

Dr. James's powder is a medicine which has been fo long in ufe, and is fo defervedly ranked among the moft valuable we poffefs, that every attempt to render the procefs for preparing it more fimple and more certain, muft be allowed to be of fome importance. But, whatever reafon there was to think, by arguing upon its chymical properties, that I had really fucceeded in improving its medicinal virtues, it ftill remained to be proved, by actual experiment, that the hoped-for fuccefs was not merely conjectural. To afcertain this, I gave fome of my powder to Dr. Crichton, Dr. Babington, and Mr. Abernethy; gentlemen whofe extenfive practice and acknowledged fkill fufficiently enabled them to judge of its medical properties. They all concur in opinion, that, in its general effects, it agrees with Dr. James's powder and the *Pulvis Antimonialis*; but, that it is more mild, and confequently may be given in larger quantities, feldom producing naufea or vomiting, in dofes of lefs than eight or ten grains.

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V.

Conſtruction of an Hydraulic Apparatus, which by Means of the Syphon raiſes Water above its level, and performs its Alternations without Attendance. By Mr. Wm. CLOSE.

To Mr. NICHOLSON.

SIR,

Dalton, Nov. 6, 1801.

AT the time when I wrote to you on the application of the ſyphon to raiſe water above the reſervoir, I could find no eaſy method of making the inſtrument afford an equable ſupply without attendance. Since then, however, I have thought of a combination for the purpoſe, applicable to any height under an elevation of thirty feet; and another to raiſe water below the reſervoir, independant of the preſſure of the atmoſphere. I ſend you a drawing, and deſcription of each.

New method of raiſing water by the ſyphon.

Plate III. exhibits a repreſentation of the apparatus for raiſing water above the reſervoir. An air pipe is inſerted into one ſide of the turn of the ſyphon S S, and a ſmall plate is fixed on the ſide of its aperture oppoſite the ſhorter branch to obſtruct part of the current. At ſome diſtance from the ſyphon this pipe divides into two branches, 2, 3, which lead to two veſſels A and B. At the ſeparation is fixed a cock of ſuch a conſtruction, that the air pipe 1 can only communicate with one of its branches at once. The barrel of the cock, or that part which is repreſented by ſhort radial lines in the ſection of this part of the apparatus, has three lateral openings: the plug or key which moves within the barrel contains a duct or channel, which, inſtead of paſſing ſtraight through it, is turned with ſuch a bend, that when one end of it is under the opening of the air pipe, the other will be oppoſite the orifice of one of the pipes 2, 3. The communication from the air pipe to either of its branches is therefore completed by about a quarter of a turn.

Drawing and deſcription of apparatus.

The veſſels A and B have each a pipe with a valve at its lower end, connected with the ciſtern C C, to carry up water, and two other pipes, one to let out the raiſed water, and the other to take in air at the ſame time. The air pipe need only be

be very ſmall, but the water pipe muſt be wide enough to empty the veſſel in the ſame ſpace of time that it was filled. Each has a valve, but as they are all upon the ſame level, only the valves of the two water pipes are repreſented. All theſe valves are cloſed by weights. Between theſe veſſels is repreſented a wheel, with a weight ſuſpended on one ſide, and the veſſel E on the other. Its rim has a groove, for the chains or cords, and one ſide is formed into teeth, to act upon another toothed veſſel above, which moves the cock in its axis.

The veſſel D receives water from the ciſtern C C, through an aperture of a proper width, and is emptied by a ſyphon as often as the water riſes to a certain height.

The ſyphon S S is about ſix inches in diameter; its ſhorter branch riſes thirty feet above the reſervoir, and its longer branch reaches five or ſix feet below the ſurface of the water.

Its effects. Suppoſe now that this inſtrument is ſet to work and ſupplied with a ſufficient quantity of water. The cock in the air pipe 1 above the veſſel A muſt be opened, and the vertical pipes A C, and B C, muſt be filled with water, by opening a very narrow paſſage between the air pipe and each of its branches, while the valves of the veſſels are cloſed.

This mode of proceeding is adviſeable to prevent the ſudden preſſure of the atmoſphere from forcing the air out of the empty pipes, with ſuch rapidity as would deſtroy the operation of the ſyphon. After this, if all the valves are in good order, and all parts of the apparatus complete, no further attention will be neceſſary.

The periphery of the lower wheel moves between the valves of the veſſel A on one ſide, and thoſe of B on the other, and has a claw or pin to depreſs each in due order. The weight ſuſpended upon the lower wheel opens the valves of B, and by the aſſiſtance of both wheels opens a communication between the air pipe 1, and the veſſel A, whoſe valves have previouſly cloſed their pipes. The preſſure of the atmoſphere accordingly fills A with water, and the air it contained is carried down the ſtream through the longer branch of the ſyphon. About this time the veſſel D is filled to a level with the bend of its ſyphon which conveys water in the veſſel E. This laſt veſſel being filled, overbalances the weight, and moves the two wheels; the valves of the veſſel B are cloſed by their reſpective weights, and the rarefying tube is made to

to communicate with the branch 3; the aperture of the key duct, which before correſponded with the end of the branch 2, is now placed under the orifice of the rareſying tube, and the opening which was there before, is turned to the branch 3.

At the time when all communication between the veſſel and ſyphon is interſected, one of the claws or pins on the farther ſide of the lower wheel, opens the valve of the air pipe in the veſſel A, and immediately after it the valve of the pipe for letting out water is depreſſed by a claw on the nearer ſide. This laſt valve will make little reſiſtance, as the whole preſſure of the atmoſphere was removed by opening the ſmall pipe firſt; for this purpoſe it has a ſeparate valve.

The raiſed water flows out of A while B is filling.

When the veſſels D and E are emptieſt, the large weight moves the wheels and the cock; the valves of B are opened in the ſame order as thoſe of A, which being again connected with the ſyphon, is filled while B is emptied.

In actual work the receptacle of raiſed water muſt be placed under the veſſels, the axis of the lower wheel may extend acroſs the receptacle, and have two arms or levers, fixed in a proper poſition to depreſs the valves, while the wheel itſelf will move on the outſide. The key or plug of the cock turned by the axis of the higher wheel, ſhould move very true in its barrel. The higher wheel ſhould be made light if the key of the cock is fixed faſt to its avis. The key or plug ſhould be frequently taken out and rubbed with tallow; its tightneſs may be adjuſted by a ſcrew fixed in a proper frame and moved by a weight. When any repair is requiſite, the cock in the air pipe above A muſt be turned.

Remarks and obſervations.

A ſyphon ſix inches in diameter will require its air pipe to be about one inch and a half in diameter.

By a ſyphon of theſe dimenſions water may be raiſed to the height of ſixty feet, if the vertical water pipe of the veſſel B, be placed in the ciſtern which receives the raiſed water from A, and the branch of the air pipe 3 be carried to the top of the veſſel B. Both the veſſel and the air pipe ſhould be made ſomething leſs on account of the greater body of air to be rarefied. The air pipe ſhould reach conſiderably above the veſſel, or it muſt have a float valve, to prevent any waſte of the raiſed water.

If

If the lower end of the pipe B C was inferted into the lower fide of the veffel A, the water might be forced out of this veffel into the higher, by admitting the preffure of the atmofphere. The higher water pipe however, fhould defcend confiderably below the bottom of the veffel, and its valve fhould be always kept in good order, or part of the raifed water will be drawn down again when the air in the lower veffel is rarefied. Indeed if we can infure the tightnefs of the valves, water may be carried through any number of veffels, without being delivered into any ciftern before the requifite height is obtained.

Conftruction of the apparatus for raifing water below the refervoir.

The apparatus reprefented in Pl. III. may be ufed to raife water below the refervoir, if the veffels and their water pipes are fufficiently fettled, and the air pipe 1 placed in a vertical pofition; but for this purpofe the combination reprefented in Pl. IV. will be preferable. It is an inverfion of the former method. A ftream of water falls down the pipe L, paffes through the veffel V, and afcends again through the fhorter pipe S. When there is a fufficiency of water to fill the longer pipe, the ftream flows through the apparatus with confiderable celerity. The longer pipe L having feveral fmall holes at the turn near the higher end, a quantity of air is drawn into the ftream, and defcends into the veffel V, where it quits the current, rifes to the higher part of the veffel, and depreffes an equal bulk of water, which is carried through the pipe S. This explains the principle.

The air however is not fuffered to collect in the veffel V, but is conveyed through the pipe 1, into one of the veffels A B, previoufly filled with water, which it forces into the ftream. The alternate connection of 1, 2, and 1, 3, is completed by a cock of the fame conftruction, as is defcribed in the explanation of Pl. III. and by the fame kind of machinery.

The lower parts of the apparatus, Pl. IV. to above the veffels A B, are immerfed in water. At prefent the veffel V communicates with the veffel A; the water will accordingly defcend out of A through the tube below 2, which has a valve opening downwards. A fmall conical valve at the lower end of a pipe inferted into the top of B, being opened immediately before the communication between V and A is completed, the condenfed air rufhes out of B, and water enters through an aperture under that letter, while the valve under 3 is clofed,

4 When

When the veſſel B is filled, and A is emptied, the cock in the air pipe begins to be put in motion, but before the tube 1 is connected to the branch above 3, the ſmall conical valve above B cloſes its air pipe; the valve under B being now cloſed by the preſſure of the water in the tubes L S, the water in B is forced down the pipe under 3, and carried away with the ſtream: the condenſed air is let out of A, and this veſſel repleniſhed with water.

The air which is carried down the ſtream while it can enter into neither veſſel above, will collect in V, until an avenue to one is completed, through which it will rapidly aſcend. If the engineer does not like the conſtruction of the cock I have recommended, its place may be ſupplied by two of the common kind, with adjuſting pieces upon their keys.

The condenſed air collected in the veſſel V, may be applied to the purpoſe of raiſing water either above the reſervoir C C, or below the veſſel V, merely by forcing the air out of V into the veſſel containing the water, by the preſſure of the water in the pipes L S.

The apparatus will begin its operations without any trouble; the capacities of the veſſels A B are indeterminate; and if the higher orifice of the pipe S be contracted, the machine will work with a leſs quantity of water than it may be calculated to receive. The veſſels A and B however, ſhould always be filled completely with water before the admiſſion of the condenſed air, otherwiſe the quantity of raiſed water will decreaſe.

A model was conſtructed to illuſtrate the principle: the two water pipes were about half an inch in diameter, and only one ſmall hole was made at the higher end of the longer. Into the top of the veſſel repreſented by V, was ſoldered two ſmall pipes, which were ſurrounded by a little thread, and inſerted into the neck of a ſmall glaſs bottle. The veſſel V and the lower parts of the water pipes were plunged into a veſſel of water, until all the air in the apparatus below the ſurface was expelled. In this ſituation the bottle, previouſly filled with water, was fixed upon the pipes above V. The model being lifted out of the water, and the longer pipe fixed to a ſmall ciſtern, the remaining parts of the water pipes were carefully filled; a ſufficient ſupply was let into the ciſtern, and the Experimental trial.

higher

higher orifice of S was opened. When the ftream had acquired its full force, bubbles of air entered the bottle, and the water was quickly expelled.

I am, SIR,

Your humble Servant,

WILLIAM CLOSE.

ERRATA in the Paper on Wind Inftruments. (Philof. J. V. 213.)

Page 217 *line* 23, erafe *major*.
—— 219 — 44, for *fifth* read fourth.
—— 221 — 11, change G, a, B, into G, A, B.

VI.

Outline of the Properties and Habitudes of the Metallic Subftance, lately difcovered by CHARLES HATCHETT, *Efq. and by him denominated Columbium.*

ON the 26th of November laft a Paper was read at the Royal Society, announcing the difcovery and inveftigation of the properties of what appears to be a new metal; and as by the courfe of publication it muft be feveral months before it will appear in the Tranfactions, I fhall prefent my readers with the following outline:

History of the mineral.

1. The mineral was fent with fome iron ores to Sir Hans Sloane by Mr. Winthrop of Maffachufets, and there is therefore every reafon to believe, that it came from fome of the iron mines in that province.

External character.

2. It is heavy, and of a dark grey nearly black; it in fome meafure has the appearance of the Siberian chromate of iron.

Habitudes with acids.

3. The nitric, muriatic, and fulphuric acids act but very feebly upon the mineral; of thefe however, fulphuric acid produces the greateft effect, and diffolves fome iron.

With potafh by fufion, and fubfequent digeftion in muriatic acid.

4. When melted with five or fix parts of carbonate of potafh it is partially decompofed; but in order to effect a complete decompofition, the ore muft be alternately melted with potafh, and digefted with muriatic acid, which takes up the iron.

5. During

5. During the fusion, carbonic acid is expelled, and the pot ash becomes partially neutralized by a metallic acid, which may be separated after solution in water, by nitric acid added to excess, and then appears in the form of a copious white flocculent precipitate. The potash holds a metallic acid; precipitable by nitric acid.

6. The ore consists of more than three fourths of this substance combined with iron. This acid very abundant

7. The white precipitate is insoluble in boiling nitric acid, and remains perfectly white. Insoluble in nitric;

8. Boiling muriatic acid dissolves it when recently separated from pot ash. But soluble in muriatic;

9. Sulphuric acid when strongly heated also dissolves it. And sulphuric acid.

10. The acid solutions when saturated by alkalis afford white flocculent precipitates; prussiate of pot ash forms an olive green ditto; tincture of galls, deep orange coloured ditto; water also when copiously added to the sulphuric solution precipitates this substance in the state of sulphate, which as it becomes dry, from white, changes to blue, and lastly to grey. Precipitable.

11. Zinc forms a white precipitate.

12. The white precipitate combines with pot ash and soda, both by the dry and humid ways; it expels carbonic acid, and with pot ash forms a glittering scaly salt, which has much the appearance of the boracic acid. Neutral salts and their affections.

13. Acids separate it from the fixed alkalis, and when added to excess do not dissolve it unless heated, but even then nitric acid has no effect.

14. The same is observed when alkalis are added in excess to the acid solutions.

15. Hydro-sulphuret of ammoniac added to the alkaline solutions forms a chocolate coloured precipitate.

16. Ammoniac will not combine with the white precipitate.

17. When prussiate of pot-ash, or tincture of galls, are added to the alkaline solutions, no effect is produced till an acid is added, and then the usual olive green and orange coloured precipitates are obtained.

18. The acid and alkaline solutions are colourless.

19. The white precipitate will not combine with sulphur in the dry way.

20. It forms a purplifh blue glafs with phofphate of ammoniac.

21. It reddens litmus paper.

22. It appears to be of extreme difficult reduction.

Conclufion.

From the above properties it appears to be an acidifiable metal different from thofe already known, and it has been therefore diftinguifhed by the name of Columbium.

VII.

On the Sebacic Acid, or Acid of Fat. By CIT. THENARD *.

Authors who have treated the fubject.

A GREAT number of chemifts have fucceffively written on the febacic acid. Grutmacher appears to have been the firft who has fpoken of it; it has fince been treated of by Rhades, Knap, Segner, and Haller, who have confirmed and augmented his experiments. Crell has given us feveral proceffes for obtaining this acid in a ftate of purity, and has fubjected it to a number of experiments, the refults of which Citizen Guyton has inferted in the *Journal de Phyfique,* (Vol. XVIII. pages 110 and 383, and Vol. XIX. page 384.) Bergman has afcertained the order of the attraction of this acid for the falifiable bafes. Citizen Guyton has handled the fame fubject, and difcovered feveral errors which had efcaped the notice of this chemift.

Object of proof by them differs confiderably from the refult of the prefent experiments.

The object of all the experiments which I have juft mentioned, is to prove that the product of the diftillation of fat contains a peculiar acid, of extreme volatility, and of fuch a poignant and fuffocating nature, that it cannot be refpired without fome degree of danger. Thofe of which I am about to give an account prove 1. That this product actually contains a peculiar acid, which however, fo far from being volatile, odorous and fuffocating, is on the contrary folid and inodorous; 2. That it alfo contains acetous acid; 3. That this new acid has no part in the fmell of diftilled fat; 4. That by all the proceffes that have hitherto been employed for extracting the febacic acid, nothing is obtained but a foreign acid, and that confequently the febacic acid has not yet been known.

* Communicated to the National Inftitute of France, and inferted in the Annales de Chimie, XXXIX. 193.

A. *Proceſſes for obtaining the Sebacic Acid, and Characteriſtic Properties of this Acid.*

HAVING diſtilled a conſiderable quantity of the fat of pork, I treated the product, at ſeveral times, with hot water, and poured acetite of lead into the liquor; a flaky precipitate was formed, which I collected and dried, after which I put it, together with ſulphuric acid, into a retort and heated it. The liquid of the receiver had no character of acidity; but in the retort a melted ſubſtance floated at the top, analogous to fat, which I carefully ſeparated, and after having waſhed it well, boiled it with water. By the aid of the action of the heat, the water totally diſſolved it, and by refrigeration cryſtalline needles were depoſited, having little confiſtence: theſe needles were acid, and poſſeſſed properties altogether peculiar to themſelves. In order to aſſure myſelf that they were not the product of the ſulphuric acid, I treated diſtilled fat with water; and having filtrated and evaporated the liquor, I obtained needles poſſeſſing preciſely the ſame properties.

Pork fat was diſtilled, waſhed with hot water, and acetite of lead poured in. The precipitate was dried and then diſtilled with ſulphuric acid. No acid came over; but a matter reſembling fat, floated in the retort. This being boiled with water proved ſoluble and cryſtallized by cooling. It is a peculiar acid; and may be obtained by waſhing diſtilled fat with water, without the uſe of ſulphuric acid.

In order to procure this acid, we may alſo, if we think fit, after having treated the diſtilled fat with water, ſaturate the filtrated liquor with pot aſh, evaporate it, and pour into it a ſolution of lead. A precipitate is formed, which is a ſebate of lead, that is to be treated as above with the ſulphuric acid. Theſe are the three methods which may be employed in order to obtain the ſebacic acid: its properties are the following:

On the aqueous ſolution from diſtilled fat may be ſaturated with pot aſh, then evaporated and decompoſed by acetite of lead. The ſebate of lead may then be made to yield its acid by the ſtronger attraction of the ſulphuric.

It is without ſmell; its taſte is ſlightly acid: it melts like a kind of fat; it conſiderably reddens the tincture of Tourneſol; it is much more ſoluble in a hot than in a cold temperature. Boiling water, ſaturated with the ſebacic acid, becomes ſolid by refrigeration; alcohol diſſolves a large quantity of it; it cryſtallizes in ſmall needles; by uſing proper precautions, we may obtain it in the form of long, large, and beautiful lamellæ, of a very brilliant appearance; it precipitates the acetite and the nitrate of lead, the nitrate of ſilver, the acetite, and the nitrate of mercury; it ſaturates the cauſticity of the alkalies, with which it forms ſoluble ſalts; with pot aſh it produces a ſalt which does not attract moiſture from the atmoſphere, which has but little taſte, and which, if we pour upon it ſulphuric, nitric, or muriatic acid, becomes turbid, and depoſits ſebacic

Sebacic acid has no ſmell; is ſlightly ſour; melts like fat; reddens turnſol; is more ſoluble in hot than cold water, and ſo much in the latter as to become ſolid by cooling. Alcohol plenti-

fully diſſolves it. Cryſtallizable. Its peculiar attractions and habitudes.

ſebacic acid; when its ſolution is concentrated and mixed with one of theſe acids, it becomes ſolid; finally it does not render turbid the waters of lime, barytes and ſtrontites. Theſe properties diſtinguiſh it from all the other acids, and evidently prove that it is an acid *ſui generis.*

B. *Means of ſeparating the Acetous Acid from the Product of the diſtillation of Fat.*

The aqueous ſolution from diſtilled fat contains acetous or ſebacic acid, more largely the greater or leſs the heat of diſtillation. The former is ſeparated by neutralizing with potaſh, and diſtilling with ſulphuric acid.

THE product of the diſtilled fat is treated with water; the liquor is ſaturated with potaſh, and evaporated. When the ſubſtance is dry, it is introduced into a retort with dilute ſulphuric acid, or phoſphoric acid, and diſtilled: an acid is obtained, which has all the characters of acetous acid; with potaſh it forms a foliated ſalt. This ſalt is ſuſceptible of being melted by means of heat; when expoſed to the air, it deliqueſces ſpeedily and completely; its taſte is extremely pungent; with a ſolution of nitrate of mercury, it forms a precipitate cryſtallized in the form of ſpangles. When ſulphuric acid is poured upon it, acetous acid is diſengaged in abundance. Sometimes the water with which the product of the diſtilled fat has been treated, contains hardly any thing elſe than acetous acid, ſo that in order to obtain acetite of potaſh, nothing more is neceſſary than to evaporate the liquid. The quantity of ſebacic and of acetous acid formed in the diſtillation of fat, varies in proportion to the leſſer or greater degree of heat.

C. *Examination of the Odorous Matter, and of the diſtilled Fat.*

The pungent odour of diſtilled fat is not produced by an acid.

I INTRODUCED fat, which had juſt been diſtilled, and the ſmell of which was extremely pungent, into a tubulated retort. I adapted to the neck of the retort, a receiver which contained tincture of Tourneſol. Having diſtilled with a gentle heat, I thereby filled the receiver with a ſtrong odour, and yet the colour of the tincture was not changed—a convincing proof that the ſmell of diſtilled fat is not owing to an acid; beſides, if this ſmell depended upon an acid, it would probably diſappear upon placing it in contact with the alkalies, as the acid would then be abſorbed; but this does not happen. We muſt therefore conclude, that it depends upon a portion of the fat, converted into gas, and undoubtedly changed in its nature.

D. *Examination*

D. *Examination of the Processes that have hitherto been employed for obtaining Sebacic Acid.*

CRELL, in order to separate the sebacic acid from the product of distilled fat, first added to it a certain quantity of potash, after which he filtrated and evaporated it. As the salt which he obtained was mixed with oil, he calcined it, after which he re-dissolved it in water, and evaporated the solution. By this means he obtained a salt of considerable whiteness, and of a foliated texture; this he introduced into a retort, with sulphuric acid, and distilled it, whereby he obtained a pungent and fuming acid. But as this process appeared to him to be an inconvenient one, he had recourse to the following:

Crell's process by lixiviation of fat with potash, filtration, evaporation, calcination, and re-solution of the salt; and lastly distillation with sulphuric acid.

Persuaded that the acid of fat existed ready formed in the fat itself, and was not a product of distillation, he prepared a soap with fat and potash, and treated this soap with water, in order to dissolve the sebate of potash formed in it. But as the water, besides the sebate of potash, dissolved also a certain quantity of fat combined with potash, he added to the solution of this sebate of potash and of this soap, a sufficient quantity of alum. By this means he obtained sebate of potash, which had only an admixture of sulphate of potash; this he evaporated, poured sulphuric acid upon the dry substance, applied heat; and sebacic acid passed into the receiver.

Another by forming soap and treating with water, and some alum and distilling as before.

In the chemistry of Dijon, we find a process different from that of Crell. According to this process, the fat is calcined with a certain quantity of lime, in a crucible; the substance is then lixiviated with a large quantity of water; the water which holds the calcareous sebate in solution, is evaporated; this calcareous sebate is introduced into a retort with sulphuric acid, and the sebacic acid passes into the receiver.

Dijon process. Calcination of fat with lime, lixiviation, evaporation, and distillation with sulphuric acid.

These three processes I have repeated with the utmost care, and have obtained the following results. The first of Crell's processes, and that of the chemistry of Dijon, afforded me an acid which has all the characteristics of the acetous acid: with potash it forms a foliated salt, which is deliquescent, has an extremely pungent taste, and on being treated with sulphuric acid, yields a large quantity of vinegar. If Crell, as he asserts, has obtained a fuming and pungent acid, I presume that it is a small quantity of sulphureous acid, proceeding from the

Crell's first process afforded acetous acid.

decomposition

decompofition of a certain portion of fulphuric acid by the fat, or of the carbone of the acetous acid difengaged from its combination.

His fecond by mifmanagement gave muriatic acid.

In following the fecond procefs defcribed by Crell, we do not obtain any acetous acid, but an acid, which is nothing elfe but the muriatic. In fact, it forms with the nitrate of filver a precipitate infoluble in an excefs of nitric acid; with foda it yields cubic cryftals. If we pour fulphuric acid upon thefe cryftals, a penetrating gas is difengaged, which, on being brought into contact with the air, gives rife to vapours: the fame acid, mixed with nitric acid, diffolves gold. With the oxide of mercury it forms a volatile falt; with potafh it forms a falt capable of being fufed without undergoing decompofition. Thefe circumftances render it probable that Crell has employed the potafh of commerce, which always contains muriate of potafh; for in repeating this procefs with pure potafh, no acid is obtained, except an almoft imperceptible quantity of vinegar. This vinegar is formed by treating the fat with potafh and the fulphuric acid; for fat contains no acid, not even when it is rancid. At leaft, I have feveral times treated rancid fat with water, and have uniformly obtained a liquid, which did not redden the tincture of Tournefol.

But if well performed, vinegar.

E. *Recapitulation.*

Enumeration and comparifon of the above refults.

THESE experiments prove, in my opinion, what I have advanced in the beginning of this memoir; namely, that there exifts in the product of diftilled fat a peculiar acid, which, inftead of being volatile, odorous, fuffocating, is on the contrary folid, inodorous, and fixed to a certain degree; that, befides this acid, the product of diftilled fat contains acetous acid; that the febacic acid has no fhare in producing the fmell of diftilled fat, which probably depends upon fome particles of fat, converted into vapour and altered in their nature. They prove, befides, that by the proceffes of Crell, and by that defcribed in the chemiftry of Dijon, we obtain only the muriatic or the acetous acid; that confequently the febacic acid has hitherto remained unknown, and that in the prefent ftate of our knowledge it is a new acid.

Hog's lard only was ufed.

I ought to mention, that all the experiments which I have related have been made with hog's lard, and that I have not, like Crell, varied thefe experiments with human fat, the marrow

marrow of beef and tallow. I propofe to repeat them with thefe different fatty fubftances, and think, even according to the experiments of Crell, that I fhall obtain the fame refults. I have not as yet examined all the properties of the febacic acid; but it is my intention to ftudy them with care, and give an account of them to the clafs.

VIII.

Improvement in the Art of preparing Radical Vinegar. By CIT. Y. PERES, *Jun.**

THE experiment which enabled me to afcertain the real difference between the acetous and the acetic acid, and afterwards to propofe to the learned a new theory of vegetable acidification in general, may be of great fervice in the art of preparing what is termed radical vinegar; or, to fpeak more accurately, it ought totally to change the procefs ufed for preparing this fubftance.

Obfervations on the diftillation of acid from acetite of copper.

The procefs at prefent in ufe, confifts in diftilling the acetite of copper in large fpherical veffels of ftone ware. By taking the greateft care, a fmall portion is obtained of this *radical vinegar*, fo much ufed in medicine and for domeftic purpofes. The difficulty of checking the expanfion of this product, is an inconvenience which, being dependent on the nature of the procefs itfelf, has at all times been felt. Some improvements have been propofed; but as the nature of the operation was not underftood, thefe improvements produced fcarcely any advantage, and the procefs ftill remains very defective. It is eafy to obferve, that the violent heat which is ufed, is much more than fufficient for depriving the acetous acid of its carbon, and that it muft reduce a portion of it to its firft elements. I do not hefitate to affert, that at leaft half the radical vinegar is loft: The large quantity of gas that is obtained, incontrovertibly proves the truth of this affertion.

Radical vinegar is deduced to be acetous acid deprived of carbon.

I have formerly fhewn, by various deductions from earlier experiments, that *radical vinegar* is nothing but acetous acid deprived of carbon. I have fhewn, that the ordinary procefs

* In a letter to the Editor of the Mag. Encyclop.

for

for preparing this acid effects nothing more than to abſtract this exceſs of carbon; and I have completed my proofs, by preparing this acid myſelf, by a means which every one knows can have no other effect than to abſtract carbon from the ſubſtances to which it is applied.

Simple proceſs for making it. One part of ſulphuric and two of vinegar are diſtilled.

I diſtilled a kilogramme of ſulphuric acid with two kilogrammes of good white vinegar. I ſuddenly brought the mixture to ebullition, and obtained a very large quantity of radical vinegar, as white and pungent as that of commerce. This proceſs is ſo ſimple and economical, that I thought it would be uſeful to extract it from the Memoirs which I have given upon this branch of chemiſtry, and offer it to manufacturers. I can aſſure them, that it will diminiſh the expences of the manufacture by three fourths. In fact, the ſulphuric acid which remains may ſtill ſerve for two more operations: but then it will be neceſſary to rectify the radical vinegar, for it will be found impregnated with ſulphurous acid gas. It would be proper to try whether the action of manganeſe, which is uſed for ether, might not be applied to this rectification. I do not apprehend that this metal, in ſo high a degree of oxidation, is ſuſceptible of being attacked by the acetic acid.

Manganeſe propoſed.

Erroneous notion of the elective attractions of acetic acid.

An erroneous opinion obtains, with reſpect to the tendency of this acid to combination, which I think it incumbent upon me to refute. Chemiſts place it, in their tables of attractions, in a much higher rank than it will be found entitled to, upon an inveſtigation of its properties with the leaſt degree of accuracy. We find that it diſplaces only the carbonic, acetous and other weak acids. This error has ariſen from the appearance of ſtrength which it derives from the pungency of its ſmell. In this inſtance, however, the chemical properties of the ſubſtance are by no means proportionate to the impreſſion it makes upon our ſenſes. I ſhall add an obſervation which proves that this acid is much leſs powerful than is generally imagined; namely, that the vapour which it ſpontaneouſly emits, and which might ſeem to be its moſt acid portion, ſcarcely reddens paper tinged with tincture of tourneſol. In fact it is nothing more than a modification of hidrogen, and it takes fire like ether.

Its fumes are ſcarcely acid, &c.

IX.

*An Account of a new Eudiometer.** *By Mr.* H. Davy, *Director of the Chemical Laboratory, and Lecturer on Chemiſtry to the Royal Inſtitution.*

THE dependance of the health and exiſtence of animals upon a peculiar ſtate of the atmoſphere, and the relations of this ſtate to proceſſes connected with the moſt eſſential wants of life, have given intereſt and importance to inquiries concerning the compoſition and properties of atmoſpheric air.

Compoſition of the atmoſphere an intereſting ſubject.

This elaſtic fluid has been long known to conſiſt chiefly of oxigen and nitrogen, mingled together, or in a ſtate of looſe combination, and holding in ſolution water.

Oxigen and nitrogen.

A variety of proceſſes have been inſtituted with the view of determining the relative proportions of the two gaſes, but moſt of them have involved ſources of inaccuracy; and lately all, except two (the ſlow combuſtion of phoſphorus, and the action of liquid ſulphurets), have been generally abandoned.

The eudiometric proceſſes commonly uſed are exceptionable.

Both phoſphorus and ſolution of ſulphuret of potaſh abſorb the whole of the oxigen of atmoſpheric air at common temperatures, and they do not materially alter the volume, or the properties of the reſidual nitrogen; but their operation is extremely ſlow; and in many caſes it is difficult to aſcertain the period at which the experiment is completed.

I have lately employed as an eudiometrical ſubſtance the ſolution of green muriate, or ſulphate, of iron, impregnated with nitrous gas; and I have found that it is in ſome reſpects ſuperior to many of the bodies heretofore uſed, as it rapidly condenſes oxigen without acting upon nitrogen; and requires for its application only a very ſimple and a very portable apparatus.

Solution of muriate, or ſulphate, of iron, impregnated with nitrous gas, propoſed to abſorb oxigen.

This fluid is made by tranſmitting nitrous gas through green muriate, or ſulphate, of iron, diſſolved to ſaturation in water†. As the gas is abſorbed, the ſolution becomes of a deep olive brown, and when the impregnation is completed it appears

How made.

* Journals of the Royal Inſt. p. 45.

† Dr. Prieſtley firſt obſerved this proceſs: for a particular account of it, ſee Reſearches, Chemical and Philoſophical, p. 152. Johnſon.

opaque

opaque and almoſt black. The proceſs is apparently owing to a ſimple elective attraction; in no caſe is the gas decompoſed; and under the exhauſted receiver it aſſumes its elaſtic form, leaving the fluid with which it was combined unaltered in its properties.

Eudiometer tube.

The inſtruments neceſſary for aſcertaining the compoſition of the atmoſphere, by means of impregnated ſolutions, conſiſt ſimply of a ſmall graduated tube, having its capacity divided into one hundred parts, and greateſt at the open end; and of a veſſel for containing the fluid.

Manipulation.

The tube, after being filled with the air to be examined, is introduced into the ſolution; and, that the action may be more rapid, gently moved from a perpendicular towards a horizontal poſition. Under theſe circumſtances the air is rapidly diminiſhed; and, in conſequence of the dark colour of the fluid, it is eaſy to diſcover the quantity of abſorption. In a few minutes the experiment is completed, and the whole of the oxigen condenſed by the nitrous gas in the ſolution in the form of nitrous acid.

Maximum of diminution.

In all eudiometrical proceſſes with impregnated ſolutions, the period at which the diminution is at a ſtand muſt be accurately obſerved; for, ſhortly after this period, the volume of the reſidual gas begins to be a little increaſed, and, after ſome hours, it will often fill a ſpace greater by ſeveral of the hundred parts on the ſcale of the tube, than that which it occupied at the maximum of abſorption.

Subſequent increaſe; and why.

This circumſtance depends upon the ſlow decompoſition of the nitrous acid (formed during the experiment), by the green oxide of iron, and the conſequent production of a ſmall quantity of aeriform fluid (chiefly nitrous gas)*; which, having no affinity for the red muriate, or ſulphate, of iron produced, is gradually evolved, and mingled with the reſidual nitrogen.

The muriate is preferable to the ſulphate.

The impregnated ſolution with green muriate is more rapid in its operation than the ſolution with green ſulphate. In caſes

* The decompoſition of nitrous acid, by ſolutions containing oxide of iron, as its minimum of oxidation, is a very complex proceſs. The green oxide, during its converſion into red oxide, not only decompoſes the acid, but likewiſe acts upon the water of the ſolution; and ammoniac is ſometimes formed, and ſmall portions of nitrous oxide and nitrogen evolved with the nitrous gas.

when

when these salts cannot be obtained in a state of absolute purity, the common or mixed sulphate of iron may be employed. One cubic inch of moderately strong impregnated solution is capable of absorbing five or six cubic inches of oxigen, in common processes; but the same quantity must never be employed for more than one experiment. Effect.

A number of comparative experiments, made on the constitution of the atmosphere at the Hotwells, Bristol, in July, August, and September, 1800, with phosphorus, sulphurets of alkalis, and impregnated solution, demonstrated the accuracy of the processes in which the last substance was properly employed. The diminutions given by the sulphurets were indeed always greater by a minute quantity than those produced by phosphorus and impregnated solutions: but the reason of this will be obvious to those who have studied the subject of eudiometry. In no instance was it found 100 parts in volume of air contained more than 21 of oxigen: and the variations connected with different winds, and different states of temperature, moisture, &c. were too small, and too often related to accidental circumstances, to be accurately noticed. Comparative experiments, or trials.

In analysing the atmosphere in different places, by means of impregnated solutions, I have never been able to ascertain any notable difference in the proportions of its constituent parts. Air, collected on the sea at the mouth of the Severn, on October the 3d, 1800, which must have passed over much of the Atlantic, as the wind was blowing strong from the west, was found to contain 21 per cent. of oxigen in volume; and this was nearly the proportion in air sent from the coast of Guinea, to Dr. Beddoes, by two surgeons of Liverpool. No remarkable difference observable in the air of various places as to its oxigen;

If we compare these results, with the results gained more than twenty years ago, by Mr. Cavendish, from experiments on the composition of atmospherical air, made at London and Kensington; considering, at the same time, the researches of Berthollet in Egypt and at Paris, and those of Marti in Spain, we shall find strong reasons for concluding, that the atmosphere, in all places exposed to the influence of the winds, contains very nearly the same proportions of oxigen and nitrogen: a circumstance of great importance; for, by teaching us that the different degrees of salubrity of air do not depend upon differences in the quantities of its principal constituent parts, it ought to induce us to institute researches concerning the different —and consequently its relative salubrity depends on substances dissolved or suspended in it.

ferent ſubſtances capable of being diſſolved or ſuſpended in air, which are noxious to the human conſtitution: particularly as an accurate knowledge of their nature and properties would probably enable us, in a great meaſure, to guard againſt, or deſtroy, their baneful effects.

X.

Analyſis of a newly diſcovered Mineral reſembling the Hyacinth. By Profeſſor TROMSDORFF. *

History. THROUGH the kindneſs of Prince Gallitzin, I received a new mineral ſubſtance, denominated by him *red garnet* (rather granat), which greatly reſembled the hyacinth in reſpect of colour, hardneſs, ſpecific gravity, &c. The native country of this new foſſil is Greenland. I ſhall call it *compact hyacinth* (dichter hyacinth.)

Action of heat. In order to inveſtigate the nature of this foſſil, it was ignited intenſely for two hours, and then ſuffered to cool. It ſuffered no loſs of weight, nor was its beautiful red colour in the leaſt impaired. It was ſubmitted to a ſimilar action of caloric ſix different times, but no change was effected. It was then reduced to a powder.

Powder boiled with potaſh, and then fuſed. A. Two hundred grains of the impalpable powder were boiled for ſome time in a ſilver crucible, with a ſolution of potaſh, containing 500 grains of dry alkali. The fluid, after having been boiled for ſome time upon the foſſil, acquired a yellow colour, and at laſt became green. It was evaporated to dryneſs, re-diſſolved in diſtilled water, evaporated again, and laſtly ignited at as high a temperature as the ſilver crucible would endure.

Maſs, difficultly ſoluble in water, depoſits a red powder. B. The fuſed maſs of the foregoing proceſs, when cold, was of a dark green colour, inclining to a paler green on the ſides which had been in contact with the crucible. Water ſeemed to have little action upon it, but after having been boiled in this fluid for a conſiderable time, it began to ſoften, and was at laſt diſſolved. The clear ſolution obtained in this manner, after having been ſuffered to ſtand undiſturbed, depoſited a

* Tranſlated from Crell's Chem. Annalen, 1801, Part VI. page 1.

reddiſh

reddiſh brown powder, greatly reſembling the red ſulphurated oxide of antimony. The ſupernatant fluid was perfectly tranſparent.

C. The whole fluid was then warmed a little, and muriatic acid added to it gradually. The powder before ſeparated became thus ſoluble after this acid had been added in exceſs. It now acquired a dark green colour, and was perfectly tranſparent. Addition of muriatic acid completes the ſolution.

D. Having concentrated this fluid down to two ounces, no ſiliceous earth being depoſited, it was evaporated to perfect dryneſs. The reſidue obtained was of a white colour. After being ignited, it weighed 110 grains. It was boiled again with pure potaſh in a ſilver crucible, evaporated to dryneſs, and fuſed. The fuſed maſs was again diſſolved in boiling water, muriatic acid was added in exceſs, and the whole evaporated to dryneſs. On adding ſome diſtilled water to the reſidue, an inſoluble powder was obtained. This powder, after being waſhed in boiling diſtilled water, was dried, and then ignited. Its weight amounted to 100 grains. *It was pure ſiliceous earth.* Evaporation to dryneſs, fuſion with potaſh, ſolution and evaporation, as before, and the addition of water, ſeparated much *ſilex.*

E. The fluid from which the foregoing product had been obtained, together with all the water expended in waſhing it, was collected together, evaporated, and mixed, whilſt *boiling hot,* with a ſolution of muriate of potaſh. A reddiſh brown precipitate fell down, which, being collected upon a filtre, was waſhed in diſtilled water. Precipitate, from laſt ſolution, by muriate of potaſh;

F. The precipitate, after having acquired a conſiderable degree of dryneſs on the filtre, was boiled during four hours in a concentrated ſolution of pure potaſh. The fluid was then attenuated with a conſiderable quantity of boiling diſtilled water, and transferred upon a filtre, of which the weight had been previouſly aſcertained. The reſidue obtained, after a repeated ebulition with boiling diſtilled water, was ſuffered to dry, and then ſtrongly ignited. Its weight was 32 grains. It was marked X. was dried, boiled in potaſh, then diluted, ſeparated, and ignited.

G. The aqueous ſolution of potaſh, together with all the water uſed for waſhing the laſt obtained products, was concentrated by evaporation. On adding muriatic acid to it, a precipitate enſued; an exceſs of acid was therefore added, in order to re-diſſolve it. The ſolution was then decompoſed by means of carbonate of ammmonia. The precipitate now ob- The laſt ſolution was ſuper-ſaturated with muriatic acid, and precipitated by carbonate of ammonia. It gave *alumine.*

 tained

tained was of a white colour. After being collected on a filtre, washed, and dried, its weight amounted to 56 grains. On dissolving it in sulphuric acid, and adding a minute quantity of potash, pure acidulous sulphate of alumine and potash was obtained. It was therefore pure *alumine*.

The precipitate X, ignited with potash and washed, was partly soluble in sulphuric acid.

H. The residue marked (X F) was now thoroughly dry, and of a considerable hardness. Its colour was a glittering reddish brown. It was reduced to powder, and digested in liquid ammonia. This alkali, however, after having being examined, had taken up nothing. Concentrated sulphuric acid being presented to a small quantity of it, had likewise no effect upon it. It was therefore mixed with a small quantity of dry potash, and ignited for some time; then dissolved in distilled water, and transferred upon a filtre. The residue was repeatedly washed. After being considerably dry, it was put into a glass evaporating bason, pure sulphuric acid was poured over it, diluted with a little water, and the whole carefully evaporated to dryness. On being suffered to cool, and distilled water being added, part of it became dissolved, and another part remained insoluble in the form of a brownish powder, which was collected on a filtre.

The solution contained *iron*.

I. This sulphuric solution (H) was of a greenish hue; its taste was astringent. Prussiate of potash threw down prussiate of iron. Tincture of galls produced a black precipitate with it. Liquid ammonia occasioned a reddish brown precipitate, which, after being washed, dried, and ignited, weighed twelve grains. It was an *oxide of iron*. It was assayed with super-saturated borate of soda by means of the blow-pipe, for investigating the presence of manganese, but no vestige of this metal could be discovered.

The sulphuric solution, H. gave (by carbonate of potash) a precipitate of *zircone*.

K. The product obtained by means of sulphuric acid (H), was mixed with three times its own weight of carbonate of potash, and boiled in a sufficient quantity of distilled water. A light earthy substance became separated, tinged slightly yellow, by an admixture of ferruginous matter. Being disposed to believe that this earthy substance might be zircone, it was merely collected on a filtre and washed, but *not* ignited. Its weight was twenty-five grains. It would, if it had been ignited, probably weighed only twenty grains. This earth was insoluble in potash, but soluble in nitric and acetic acid.

United

United with ſulphuric acid to perfect ſaturation, it yielded a ſalt of a difficult ſolubility; but if this acid was added in exceſs, the ſalt was readily ſoluble in water. When ignited it became hard and inſoluble in acids. I therefore do not heſitate to believe, that this earth was *zircone,* though not abſolutely pure, but ſoiled with iron.

From theſe experiments it follows that 200 grains of this hyacinth-like foſſil, conſiſt of

100	grains	ſilex (D)
56	——	alumine (G)
12	——	oxide of iron (I)
20	——	zircone (K)
12	——	Loſs of matter during the operation,
200		

XI.

Philoſophical Diſquiſitions on the Proceſſes of common Life.—Art of Shaving.—W. N.

THE caprice of faſhion, or the modern improvements in perſonal neatneſs, has deprived all the nations of Europe of their beards; and conſequently a portion by no means inconſiderable of the ſmall and daily conveniencies of life muſt depend on the facility with which this appendage can be taken off. This ſubject is frequently treated in converſation; and I am perſuaded that many individuals will be glad to know what can be ſtated on the whole reſpecting it.

Proceſs of ſhaving.

The fabrication of a good razor depends on ſo many circumſtances and conditions, the material, the art of forging, the hardening and the temper, that the artiſt himſelf after he has exerciſed his utmoſt ſkill, can in the laſt inſtance, ſelect ſuch inſtruments as are eminently ſuperior to others, by trial or actual uſe. I am not aware of any means of chuſing a good razor out of a number. All that I can ſay in this reſpect is, that a bad razor cannot be eaſily brought to a fine, or even a moderate edge; for which reaſon I ſhould prefer that razor which poſſeſſes the beſt edge, and has been moſt ſlightly touched upon the hone: that is to ſay, the razor which, upon looking

Inſtructions for chuſing a good razor.

looking along its edge, has little or no flat part where the action of the hone has taken place, and which when drawn along the hand appears keen and smooth. I must here take notice, that the common practice of cutting the skin of the hand, in order to try a fine edge, is by no means so delicate as that of placing the edge of the razor lightly on the thick skin of the hand, so as to bear upon a length of about two inches, and then drawing it along through about one quarter of an inch, without cutting. In this manner the irregularities of edge in the finest surgeon's instruments may be plainly felt.

Character of a very fine edge.

Art of giving a fine edge to cutting tools.

I will suppose the operator to be in possession of a good razor. He is in the next place to learn how to keep it in order. The original keenness of edge will necessarily go off by use. It can only be restored by whetting or grinding it up again. This is usually done by means of a good strap; in which little instruction is required besides what is common to every operation upon the edge of cutting instruments; namely, that they must never be sharpened but when actually dull, and that the operation of sharpening them is mischievous, if it be carried a moment beyond the time in which the full effect is produced. The act of strapping produces a smooth edge; but on account of the elasticity of the strap this edge becomes round and obtuse in the angle formed by its faces.

The strap.

Whetting upon the hone.

A razor which has its edge too much rounded to be revived by the strap, must be sharpened upon the hone. We may consider a hone as composed of fine sand agglutinated together, the particles of which cut or rasp away the face of the tool that may be whetted upon it. This face will accordingly become filled with scratches, and the edge will be an irregular saw, with notches, so much the finer as the particles of sand were smaller, and the pressure of rubbing more light. It is found that the action of a hone is smoother, if it be left with oil upon the face when put by; an effect which probably arises from the oil becoming glutinous, and clogging the cutting points of the sand. The principal instructions for whetting a razor are, 1. That it should be drawn lightly along the stone by repeated alternate strokes with the edge foremost; and by no means backwards and forwards, unless a considerable part be required to be whetted away, which can seldom be the case unless there be a notch, or manifest irregularity in

Particular instructions.

in the edge; 2. That the edge ſhould be tried upon the hand after every two or more ſtrokes, in order to aſcertain the inſtant at which it ſhall have become very uniformly rough. This roughneſs, if the hone be good, and the preſſure light, will conſtitute a very fine edge, though in general leſs ſmooth than is left by a good ſtrap; and 3. The edge will therefore be completed by a ſtroke or two upon ſuch a ſurface. But I will ſuppoſe that a longer continuance of whetting has been neceſſary, ſo as to produce what is called a wire, conſiſting of a very thin film of ſteel adhering along the edge. If this ſhould break off upon the hone during the whetting, the edge will become notched in paſſing over it; for which reaſon it muſt be taken off by a direct operation; namely, by paſſing the edge of the razor once along the hone, holding the back rather more upright than half way between the flat and the perpendicular poſition, and then paſſing it once back again, inclining the razor the contrary way. Theſe two ſtrokes will detach the wire, and produce a very perfect, though very obtuſe edge; which muſt then be brought up to the requiſite keenneſs by two or more light ſtrokes in the firſt mentioned method, taking care not to go too far. In both the proceſſes of ſtrapping and whetting, we have ſuppoſed the razor to be laid flat; but where a ſtrong edge is required, the whetting, but not the ſtrapping, may be performed with the back of the razor elevated ſomewhat leſs above the face of the hone, than half the breadth of the blade.

Wire upon the edge;

Taken off.

Our cutting inſtrument being now in order, there remains but one more obſervation before we proceed to the ſubject of operation; which is, that it will be found to act conſiderably better after immerſion in hot water. While I admit the fact, I muſt confeſs that I am far from being ſatisfied with any of the explanations. It has been ſaid, that the expanſion by heat enlarges the fine notches of the edge, and probably developes or opens others; but this effect can hardly be thought of any conſequence, when it is conſidered that the whole expanſion produced by boiling water does not exceed one part in ten thouſand. Others have remarked, that as heat ſoftens horn, hair, and other ſimilar ſubſtances, the hot edge may paſs through a hair more eaſily than if cold; but here we may remark, that the heat ſeems too little, and its application to the hair too momentary, to be productive of ſo conſiderable a difference;

Enquiry how far heating a razor in water is advantageous.

By improving the edge;

Or by ſoftening the hair;

difference; befides which the advantage is faid, and I believe truly, to follow even if the razor be fuffered to cool before it is ufed. Hence it fhould feem as if the hot water detached fome particles of greafe from the edge, and by that means facilitated the fliding of the cut furface of the hair upon the face of the razor.

Or by cleaning the blade.

Application and utility of foap.

There is much difference of opinion as to the application and ufe of foap. By fome it is applied cold and thick by means of a brufh; others apply it hot; and others again apply hot fuds or foap-water, with much rubbing, until the alkali has rendered the fkin much fofter, and more difpofed to be acted on by water than in its ufual ftate. Sir John Chardin afferts, that the great excellence of the Perfian barbers confifts in this practice; but I can affert on the other hand, that the Chinefe, who fhave with exquifite facility, ufe a foap box and brufh with cold water. Whether the effect of the foap be to foften the furface of the hair by an incipient combination of alkali; whether it renders the ftroke more eafy by caufing the razor to flide with facility along the furface, inftead of raifing up portions of fkin and cutting them off, as it might otherwife do, are queftions to be folved by direct experiments. Of thefe I know none, excepting that a mere folution of alkali is lefs effectual than foap, and fo likewife is the mere application of oil or fat. Hence probably we may infer, that foap acts in a twofold manner; by diffolving and removing the matter of perfpiration by its alkali, and lubricating by its oil. With regard to the difference of heat and cold, thefe effects may perhaps be forwarded, and the hair fomewhat foftened at the higher temperature, though the difference feems to be not very confiderable.

Perfian and Chinefe barbers.

Probable ufe of foap.

Mechanical procefs or operation of fhaving.

Some operators place the razor flat upon the face, and others raife it to a confiderable angle. It is certain that the procefs may be fkilfully performed either way. It is a very bad practice to prefs the razor at all againft the face; and indeed this can not be done with impunity if a drawing ftroke be ufed. Unfkilful fhavers will generally injure the fkin lefs if they lay the razor flat; but generally fpeaking, the clofeft fhaving will be performed by holding the razor at the fame inclination as was ufed in whetting it.

General Remarks.

Upon the intire view of the fubject it appears to me, that the only indifpenfable condition among the requifites before

difcuffed,

difcuffed, is that the razor fhould be in good order: and that all the other particulars may be varied, except the mechanical procefs of cutting;—that the great fecret of taking off the beard with facility, confifts in a drawing ftroke, that is to fay, that the line of the motion of the razor itfelf fhould be very oblique to the line of the edge, and not at right angles to that line, as is commonly practifed; fo that the tool may be made to exert its action as a faw, which is much more powerful than its fimple effect as a wedge. This method is indeed fo very effectual, as to require great care and confiderable practice before it can be adopted, in the extreme, with perfect fafety; but the fame efficacy which endangers the fkin, renders it eafy and pleafant with regard to the beard.

Effential part of manipulation.

XII.

Abftract of the Enquiries of CIT. BENEDICT-PREVOST, *and fome other Natural Philofophers, relative to the Motions of Odorous Subftances placed upon Water. By* CIT. BIOT *.

IT is a fact, which has long been known to natural philofophers, that fmall pieces of camphor, when placed upon water, have a very rapid rotatory motion. Volta and Brugnatelli have obtained the fame refults, by employing the benzoic and fuccinic acids. Citizen Benedict Prevoft has extended this property to a great number of odorous fubftances.

Motions of camphor, &c. upon water.

But however unanimous philofophers have been in admitting the facts, they differ greatly from each other in the explanations they have given.

Citizen Prevoft attributes thefe motions to the emanation of the odorous particles of the bodies: The experiments upon which he grounds this opinion may be found in the numbers that have been quoted. Venturi, Profeffor of Natural Philofophy at Modena, applies to thefe phenomena the explanation which Monge has given of the apparent attractions of fubftances floating upon the furface of water: according to him, "the water has a ftronger attraction for the folid camphor,

Difcuffions refpecting their caufe.

Venturi fuppofes the action of water.

* Soc. Philom. 54. The experiments of Prevoft, Venturi, and Carradori, alluded to in this notice, are given in the Philofophical Journal, 4to; for which fee the Indexes. N.

than

than for the ſmall portion of it which it has already diſſolved and ſaturated; it aſcends along the ſolid piece, and there forms a curvilinear inclined ſurface. The ſmall portion that is diſſolved and ſaturated deſcends along this ſurface, and, whilſt it is deſcending, it puſhes backwards, according to the laws of mechanics, the ſurface itſelf, and the ſolid piece that adheres to it." He thinks that we ought not to confound this effect with the repulſions which air impregnated with ether, or the exhalations of very hot camphor, exerciſes upon the light bodies which are made to float upon the ſurface of the water. In this caſe only he admits the preſence of an elaſtic fluid. *(Annales de Chimie, tome* 21.)

Dr. Carradori, a kind of oil.

Dr. Carradori is of a different opinion: He explains this motion by the elective affinity of a ſpecies of oil, which, according to him, proceeds from the camphor in contact with the water. He believes that the retreat of the water, which takes place upon a wet china plate or piece of glaſs, when camphor or other odorous ſubſtances are placed upon it, is the effect of the elective attraction of the ſurface of the plate or glaſs for the oil which the ſubſtances emit; and, according to him, it is this oil which cauſes the water to recede, by ſubſtituting itſelf in its place. *(Annales de Chimie, tome* 37.) In ſupport of his opinion, Dr. Carradori alledges, that camphor does not move upon the ſurface of the water when this is very limited. He has not been able to make ſmall plates of metal move, as Citizen Prevoſt had announced, by placing a morſel of camphor upon them, and letting them float upon the ſurface of the water. However, I have ſeveral times repeated this experiment, and always with ſucceſs; but it requires a great deal of care and extreme accuracy.

New experiments and obſervations of Prevoſt.

Citizen Prevoſt has replied to Dr. Carradori, in a Memoir which he has addreſſed to the Society, and which is intitled, *Nouvelles Expériences ſur les Mouvemens ſpontanés de diverſes Subſtances, à l'approche ou au contacte les unes des autres.* The following are the principal facts which it contains:—

Ether acts at a diſtance.

A drop of ether, placed upon a ſmall plate of tin [*fer blanc*], 15 grammes ($3\frac{1}{4}$ drachms) in weight, throws it into a lively motion, though it does not touch the ſurface of this liquid.

Thus ether acts upon water *at a diſtance.* This fact may be verified in a very ſimple manner. If we place a ſmall plate of

of tin upon water, and bring the extremity of a glafs tube wetted with ether within fome centimetres diftance of it, the plate recedes.

Camphor upon dry mercury.

Small pieces of camphor, thrown upon mercury that had been well dried, were agitated there with the fame motions as on the water. In order that this experiment may fucceed, it is neceffary that the mercury fhould be well cleaned or dried: the fmalleft particle of oil or fat fpread upon its furface ftops the motion. The pieces of camphor ought to be very fmall, the reafon of which will be feen hereafter.

Camphor upon mica floating on mercury.

Very thin plates of mica, placed upon mercury, and charged with a fmall piece of camphor, move in the fame manner as upon water.

Benzoic acid.

The benzoic acid likewife affumes a rotatory motion upon mercury; but it is neceffary that it be reduced into almoft imperceptible fragments. An oily aureola forms itfelf round thefe fragments. Nothing fimilar to this is obferved round camphor, not even when examined with the microfcope. The metallic luftre of the mercury is not impaired by it.

Water unneceffary.

It refults from thefe facts, that the prefence of water is not neceffary to the motions of odorous fubftances.

Thefe fubftances caufe the water to recede upon plates of alum, of pottery [*terre à faïance*], and of gum arabic, in the fame manner as upon a wet plate of porcelain. This retreat is not therefore owing to the elective attraction of the oily or odorous fubftance for the furface of the plate.

Finally, notwithftanding the affertion of Dr. Carradori, camphor moves in very confined veffels: Citizen Prevoft has feen it agitated in capillary tubes, into which it was introduced in very minute fragments.

Inference, that the motion is caufed by an elaftic fluid.

From thefe facts Citizen Prevoft concludes, that the intervention of an elaftic fluid is neceffary to the production of thefe phenomena. To the facts which he has adduced, I fhall fubjoin the following, which appear to me to decide the queftion relative to the motions of camphor upon water.

Experimentum crucis.

If we cut a fmall piece of camphor, of the weight of a few grains, into the form of a cone, and approach it to the diftance of four or five millemetres of a very fmall piece of leaf gold floating upon the furface of the water, prefenting the point to it, this fmall piece of gold is repelled; and we may thus conduct it through the whole extent of the vafe, without ever being

being able to touch it. It is neceffary that the water fhould be very pure, and the veffel perfectly clean. We may hold the piece of camphor with a pair of tweezers, or at the end of a glafs tube; it muft be cut into a conical form, as has been mentioned; a piece of a larger fize and an irregular form would envelope the light body in its atmofphere, and it would not move with equal facility.

The fame effects are produced, if we employ, inftead of camphor, a fmall piece of fine fponge foaked in camphorated water, or merely a tube of glafs, containing at its extremity a drop of the fame folution.

Variation. If we cover a china plate with a very thin layer of water, and approach the piece of camphor of the preceding experiment within a few millemetres of it, prefenting it by the point, in fuch a manner that the axis of the cone is perpendicular with the furface of the layer, the water recedes under the cone, and forms a circle concentric with it. The interior part of this circle is coloured with prifmatic ranges, proceeding from the prolongation of the axis, and extending from the centre towards the circumference with a very rapid motion; after fome moments, the circle lofes its colour from the centre to the circumference, and the iris finally difappears, whether we continue to hold the camphor over the furface of the layer or not. It is indifferent whether we hold the plate in a horizontal or vertical pofition: the circle is always formed in a direction perpendicular to the axis of the fmall cone of camphor. I obferved thefe phenomena at the temperature of 15° of Reaumur's thermometer.

Ether on water. Laftly, if we throw a fmall piece of fine fponge foaked in ether upon water, it is inftantaneoufly thrown into motion like camphor; and a hiffing noife is heard, fimilar to that of water during its converfion into vapour upon a hot iron. If we view the furface of the water horizontally, before a light window, we obferve fparkling jets iffuing from the fponge, which extend themfelves in ferpentine windings upon the furface of the water, to the diftance of fome centimetres, and there produce irides fimilar to thofe of the preceding experiment. Thefe irides foon difappear. During this emiffion, the fponge has a progreffive and a rotatory motion, which are evidently owing to thofe fmall jets, the impulfe of which it is obferved conftantly to obey.

Of

Of these three experiments, the two first shew that camphor acts upon water *at a distance*, and without contact; the third renders the manner in which its motions can be performed upon this liquid perceptible to the senses. Inferences.

It appears to me, that from a comparative view of these facts, we may deduce the following inferences as certain:

Camphor is moved upon the surface of water by the effect of the emission of the particles which compose it; an emission that becomes perceptible to our senses by the smell which it produces, and by the repulsions which it exercises against small bodies floating upon the surface of the water.

As the effect resulting from these different impulses does not pass through the centre of gravity of the piece of camphor, this centre has a progressive motion, and the body revolves round it. As the figure of the piece of camphor changes every moment, the motion of its centre of gravity is neither uniform nor rectilinear; it varies incessantly, as well as the angular velocity of rotation. As the evaporation takes place principally at the surface of the water, the rotatory motion establishes itself round the axis which is perpendicular to this surface, and which passes through the centre of gravity of the body.

As, *ceteris paribus*, the emanation of the particles of the camphor is proportionate to the extent of its surface, and as the surfaces increase only as the squares, but the masses as the cubes of corresponding dimensions, the rapidity of the motion of the camphor must be greater in proportion as its volume is smaller, and consequently its motion must be accelerated in proportion as it evaporates; which coincides with experience.

Having established these propositions, which in my opinion comprehend the true theory of the motions of camphor upon water, let us return to the second part of the work of Citizen Benedict Prevost.

It includes a great number of experiments, in which we see inodorous substances produce, upon wet glass, the same phenomena as odorous, oily, or volatile substances. Other experiments with bodies not odorous.

If we spread a small piece of fine wet linen, of any figure, upon a china plate moistened with a thin layer of water, the water appears to recede all round it, forming a multitude of jets, and exhibiting the prismatic colours *.

* These colours probably result from the decomposition of the light by the small layer of water which surrounds the piece of linen, and which becomes still thinner by the retreat of the water.

If

If we pour upon the piece of linen, after having ſpread it upon the plate, ſome drops of water coloured with logwood, this water runs off in coloured ſtreaks.

The ſame effects take place when we uſe a piece of fine white paper, not ſized.

We obtain them equally with all the animal and vegetable ſubſtances, with the ſaline liquids and ſolutions; whether we place them, under the ſame circumſtances, in contact with each other, or with water.

Theſe phenomena take place not only upon a wet china plate, but they are alſo obſerved upon ſurfaces of pottery, and many other ſubſtances.

From theſe facts, and ſeveral others analogous to them, Citizen Prevoſt draws the following inferences:

General reſults. 1. That all liquids poſſeſs the property of mutually repelling each other.

2. That all dry organiſed ſubſtances, which preſerve any remains of organiſation, exhale, whilſt they imbibe water, an elaſtic fluid, which carries with it a part of this water, and repels that which ſurrounds it upon a piece of wet glaſs.

The firſt inference is contradictory to the general law of the mutual attraction of the molecules of matter.

As to the hypotheſis of Citizen Prevoſt, concerning the formation of an elaſtic fluid, we ſhall remark, that, before he attributes phenomena to new cauſes, he ought to endeavour to account for them by thoſe which are already known; to diſtinguiſh the effects produced by odorous ſubſtances from thoſe preſented by inodorous ones; and perhaps to eſtabliſh in a more certain manner this repulſion of liquids by paper and linen; for this repulſion may very poſſibly be nothing more than an appearance produced by the flowing down of the water upon the inclined ſurface which theſe ſubſtances raiſe around themſelves as they imbibe this liquid.

XIII.

Case of a young Gentleman, who recovered his Sight when seven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks. By Mr. JAMES WARE, *Surgeon.* *

History of the infancy of a child blind with cataracts.

MASTER W. the son of a respectable clergyman, at Castlecary, in Somersetshire, was born in the year 1793; and, for many months, appeared to be a healthy perfect child: his eyes, in particular, were large and rather prominent. When about six months old, he began to cut his teeth; which was attended with great pain, and frequently with violent convulsive fits. About the end of his first year, a number of persons passing in procession near his father's house, accompanied with music and flags, the child was taken to see them; but, instead of looking at the procession, it was observed that, though he was evidently much pleased with the music, his eyes were never directed to the place from whence the sound came. His mother, alarmed by this discovery, was naturally led to try whether he could see silver spoons, and other glaring objects, which she held before him at different distances; and she was soon convinced, that he was unable to perceive any of them. A surgeon in the country was consulted, who, on examining the child's eyes, discovered an opacity in the pupils, which was so considerable, that he did not hesitate to pronounce there was a complete cataract in each. A description of the child's situation was then sent to me, with a request that I would point out those steps which its parents should pursue. The case was so evident, that I could not hesitate in saying, that the removal of the opaque crystalline humour, from the place it occupied behind the pupil, was the only method by which the child could obtain his sight; and, attached as I was, at that time, in all cases, to the operation of extracting the cataract, in preference to that of depressing it, I added, that I did not think he would be fit for the operation, until he was at least thirteen or fourteen years old. This advice being approved, all thoughts of assisting his sight were, for the present, relinquished. He

* Philos. Trans. 1801, p. 382.

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History of the infancy of a child blind with cataracts.

ſoon diſcovered a great fondneſs for muſic; his memory was very retentive of the little ſtories that were read or recited to him; and, in every way, it became evident that he had a mind capable of receiving information. As ſoon as he could ſpeak, it was alſo obſerved, that when an object was held cloſe to his eyes, he was able to diſtinguiſh its colour, if ſtrongly marked; but, on no occaſion, did he ever notice its outline or figure. In November, 1800, his parents took him to Briſtol; whither they went for the purpoſe of ſeeing the works carried on in the ſchool for the indigent blind in that city, and in order that they might aſcertain whether their ſon, who was then arrived to his ſeventh year, could be taught any thing that would be uſeful or amuſing. Here he very quickly learnt the art of making laces. But his parents, having brought him ſo far from home, thought it adviſeable to extend their plan, and make a viſit to the metropolis, for the ſake of giving me an opportunity of inſpecting his eyes, and of hearing whether my opinion continued the ſame as that which I had written to them ſix years before. About a month previous to the time of their arrival, a Portugueſe boy, fourteen years old, had been put under my care, who was in a ſimilar ſituation; and, in this caſe, notwithſtanding all the efforts I could uſe, I found it impoſſible to fix the eye, in order to extract the cataract, without employing a degree of force which might have been highly injurious. I therefore relinquiſhed my intention of performing the operation in that way, and determined to make uſe of the couching needle; being prepared, either to depreſs the cataract with this inſtrument, if it was ſufficiently ſolid for the purpoſe, or, if it was ſoft or fluid, (which I rather expected,) to puncture its capſule largely, ſo as to bring the opaque cryſtalline into free contact with the aqueous and vitreous humours. In order to fix the eye for this operation, I was not afraid to make uſe of a ſpeculum oculi; ſince a preſſure, which would have been highly dangerous in extracting the cataract, might be applied on the preſent occaſion with perfect ſafety. Conformably to my expectation, the cataract was of a ſoft conſiſtence; in conſequence of which, I was not able to depreſs it, and contented myſelf with making a large aperture through the capſule, by means of which the cryſtalline was brought into

Caſe of a boy who recovered his ſight by puncture of the capſule of the cryſtalline.

contact

contact with the other humours, a confiderable part of it coming forwards, and fhewing itfelf directly under the cornea.

This being the immediate refult of the operation, it could not be expected that any improvement fhould be made in the fight of the patient at that time. In a few days, however, the opaque matter was wholly abforbed: the pupils became clear; and the lad recovered the fight of both his eyes.* Encouraged by the fuccefs which followed this operation, I was induced to retract the opinion which I had formerly fent to Mafter W.'s father, (which opinion I had given under the impreffion that the cataract fhould be extracted,) and I now propofed, that an attempt fhould be made to afford relief to one eye, at leaft, without further lofs of time; this attempt, in the way above mentioned, being practicable with as much fafety at his prefent age as at any future period; and, if it proved fuccefsful, it would give the young gentleman the benefit of vifion five or fix years fooner than his friends had been encouraged to expect, by my former letter on this fubject. They were naturally much pleafed with this alteration in my advice; and the child himfelf appearing to poffefs a great degree of fortitude, I performed the operation on the left eye, on the 29th of December laft, in the prefence of Mr. Chamberlayne, F. A. S. Doctor Bradley, of Baliol College, Oxford, and Mr. Platt, furgeon, in London. It is not neceffary, in this place, to enter into a defcription of the operation. It will be fufficient to fay, that the child, during its performance, neither uttered an exclamation, nor made the fmalleft motion, either with his head or hands. The eye was immediately bound up, and no inquiries made on that day with regard to his fight. On the 30th, I found that he had experienced a flight ficknefs on the preceding evening,

The fame operation performed on the fubject of this paper, at about eight years old.

* It fhould be remarked, that the fight obtained by children who are born with cataracts, is feldom fo perfect as that which thofe recover, after the operation, who are afflicted with the diforder later in life. In confequence either of fome remaining opacity in the cryftalline capfule, which hinders the free admiffion of the rays of light, or of a greater tenuity in the remaining humours of the eye, children require, in general, a much deeper convex glafs to enable them to fee minute objects; and, at the fame time, they are obliged to hold them much nearer their eyes than older perfons.

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but had made no complaint of pain, either in his head or eye. On the 31ſt, as ſoon as I entered his chamber, the mother, with much joy, informed me that her child could ſee. About an hour before my viſit, he was ſtanding near the fire, with a handkerchief tied looſely over his eyes, when he told her that under the handkerchief, which had ſlipped upward, he could diſtinguiſh the table by the ſide of which ſhe was ſitting: it was about a yard and a half from him; and he obſerved that it was covered with a green cloth, (which was really the caſe,) and that it was a little farther off than he was able to reach. No further queſtions were aſked him at that time; as his mother was much alarmed, leſt the uſe thus made of his eye might have been premature and injurious. Upon examination, I found that it was not more inflamed than the other eye; and the opacity in the pupil did not appear to be much diminiſhed. Deſirous, however, to aſcertain whether he was able to diſtinguiſh objects, I held a letter before him, at the diſtance of about twelve inches, when he told me, after a ſhort heſitation, that it was a piece of paper; that it was ſquare, which he knew by its corners; and that it was longer in one direction than it was in the other. On being deſired to point to the corners, he did it with great preciſion, and readily carried his finger in the line of its longeſt diameter. I then ſhewed him a ſmall oblong band-box covered with red leather, which he ſaid was red and ſquare, and pointed at once to its four corners. After this, I placed before him an oval ſilver box, which he ſaid had a ſhining appearance; and, preſently afterwards, that it was round, becauſe it had not corners. The obſervation, however, which appeared to me moſt remarkable, was that which related to a white ſtone mug; which he firſt called a white baſon, but, ſoon after, recollecting himſelf, ſaid it was a mug, becauſe it had a handle. Theſe experiments did not give him any pain; and they were made in the preſence of his mother, and of Mr. Woodford, a clerk in his Majeſty's Treaſury. I held the objects at different diſtances from his eye, and inquired very particularly if he was ſenſible of any difference in their ſituation; which he always ſaid he was, informing me, on every change, whether they were brought nearer to, or carried further from him. I again inquired, both of his mother and himſelf, whether he had ever, before this

He gained his ſight,

and immediately diſtinguiſhed a table and *its diſtance*;

alſo *ſquare paper*;

a *round* ſhining *box*;

and a white *mug*.

Other experiments, by which it appeared that he knew diſtances and figures with much preciſion;

this time, diſtinguiſhed by ſight any ſort of object; and I was aſſured by both that he never had, on any occaſion; and that, when he wiſhed to diſcover colours, which he could only do when they were very ſtrong, he had always been obliged to hold the coloured object cloſe to his eye, and a little on one ſide, to avoid the projection of the noſe. No further experiments were made on that day. On the 1ſt of January, I found that his eye continued quite free both from pain and inflammation, and that he felt no uneaſineſs on the approach of light. I ſhewed him a table knife; which at firſt he called a ſpoon, but ſoon rectified the miſtake, giving it the right name, and diſtinguiſhing the blade from the handle. by pointing to each as he was deſired. He afterwards called a yellow pocket-book by its name, taking notice of the ſilver lock in the cover. I held my hand before him; which he knew, but could not at firſt tell the number of my fingers, nor diſtinguiſh one of them from another. I then held up his own hand, and deſired him to remark the difference between his thumb and fingers; after which, he readily pointed out the diſtinctions in mine alſo. Dark-coloured and ſmooth objects, were more agreeable to him than thoſe which were bright and rough. On the 3d of January, he ſaw, from the drawing-room window, a dancing bear in the ſtreet; and diſtinguiſhed a number of boys that were ſtanding round him, noticing particularly a bundle of clothes which one of them had on his head. On the ſame evening, I placed him before a looking glaſs, and held up his hand: after a little time he ſmiled, and ſaid he ſaw the ſhadow of his hand, as well as that of his head. He could not then diſtinguiſh his features; but, on the following day, his mother having again placed him before the glaſs, he pointed to his eyes, noſe, and mouth, and ſeemed much gratified with the ſight.

a dancing bear;

looking glaſs.

Having thus ſtated the principal obſervations that were made by Maſter W. I ſhall now make a brief compariſon between this ſtatement, and that which is given in the XXXVth volume of the Philoſophical Tranſactions, of Mr. Cheſelden's patient, who was ſuppoſed to be born blind, and obtained his ſight when he was between thirteen and fourteen years old,

Compariſon of this ſtatement with the famous inſtance of Cheſelden.

It

It should be obferved, that though Mafter W. was fix years younger than Mr. Chefelden's patient, he was remarkably intelligent, and gave the moft direct and fatisfactory anfwers to every queftion that was put to him. Both of them, alfo, if not born blind, loft their fight fo very early, that, as Mr. Chefelden expreffes it, "they had not any recollection of having ever feen."

My firft remark is, that, contrary to the experience of Mr. Chefelden's patient, who is ftated "to have been fo far from making any judgment of diftance, that he thought all objects touched his eyes, as what he felt did his fkin." Mafter W. diftinguifhed, as foon as he was able to fee, a table, a yard and a half from him; and proved that he had fome accuracy in his idea of diftance, by faying, that it was a little further off than his hand could reach. This obfervation, fo contrary to the account we have received of Mr. Chefelden's patient, would have furprifed me much more than it did, if I had not previoufly, in fome fimilar inftances, had reafon to fufpect that children, from whom cataracts had been extracted, had a notion of diftance the firft moment they were enabled to fee. In the inftance particularly of a young gentleman from Ireland, fourteen years old, from each of whofe eyes I extracted a cataract, in the year 1794, in the prefence of Dr. Hamilton, phyfician to the London Hofpital, and who, before the operation, affured me, as did his friends, that he never had feen the figure of any object, Dr. Hamilton and myfelf were much aftonifhed by the facility with which, on the firft experiment, he took hold of my hand at different diftances, mentioning whether it was brought nearer to, or carried further from him, and conveying his hand to mine in a circular direction, that we might be the better fatisfied of the accuracy with which he did it. In this cafe, however, and in others of a like nature, although the patients had been certainly been blind from early infancy, I could not fatisfy myfelf that they had not, before this period, enjoyed a fufficient degree of fight to imprefs the image of vifible objects on their minds, and to give them ideas which could not afterwards be entirely obliterated. In the inftance of Mafter W. however, no fufpicion of this kind could occur; fince, in addition to the declaration of himfelf and his mother, it was proved by the teftimony of the furgeon

Other inftances differing from Chefelden.

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who examined his eyes in the country, that the cataracts were fully formed before he was a year old. And I beg leave to add further, that on making inquiries of two children, between seven and eight years of age, now under my care, both of whom have been blind from birth, and on whom no operation has yet been performed, I find that the knowledge they have of colours, limited as it is, is sufficient to enable them to tell whether coloured objects be brought nearer to, or carried further from them, for instance, whether they are at the distance of two inches or four inches from their eyes; nor have either of them the slightest suspicion, as is related of Mr. Cheselden's patient, that coloured objects, when held before them, touch their eyes.

But the judgment which Master W. formed of the different distances of objects, was not the only instance in which he differed from Mr. Cheselden's patient; who, we are informed, "did not know the figure of any thing, nor any one thing from another, however different in shape and magnitude;" for Master W. knew and described a letter, not only as white, but also as square, because it had corners; and an oval silver box, not only as shining, but also as round, because it had not corners: he likewise knew, and called by its name, a white stone mug, on the first day he obtained his sight, distinguishing it from a bason, because it had a handle. These experiments were made in the presence of two respectable persons, as well as myself; and they were several times repeated, to convince us that we could not be mistaken in them. I mention the circumstance, however, with much diffidence, being aware that the observations not only differ from those that are related of Mr. Cheselden's patient, but appear, on the first statement, to oppose a principle in optics, which I believe is commonly and justly admitted, that the senses of sight and feeling have no other connection than that which is formed by experience; and, therefore, that the ideas derived from feeling can have no power to direct the judgment, with respect either to the distance or form of visible objects. It should be recollected, however, that persons who have cataracts in their eyes, are not, in strictness of speech, blind, though they are deprived of all useful sight. The instances I have adduced prove, that the knowledge they have of colours is sufficient to give them some idea of distance, even in their

Remarks on figures, &c.

darkest

darkeſt ſtate. When, therefore, their ſight is cleared by the removal of the opaque cryſtalline, which intercepted the light, and the colour of objects is thereby made to appear ſtronger, will it be difficult or unphiloſophical, to conceive that their ideas of diſtance will be ſtrengthened, and ſo far extended as to give them a knowledge, even of the outline and figure of thoſe objects with the colour of which they were previouſly acquainted?

Chirurgical obſervations and remarks.

The caſe which I have here related appears to deſerve notice, not only on account of the obſervations that were made by the patient on recovering his ſight, but alſo on account of the hint which it affords to ſurgeons, relative both to the mode in which the cataract may beſt be removed, when children are born with this diſorder, and the time when it is moſt proper to perform the operation.

In what caſes depreſſion of the cataract may be preferable to extraction.

The Baron de Wenzel, in his ingenious Treatiſe on the Cataract, with great force of reaſoning, deduced from the long and ſucceſsful experience of his father and himſelf, recommends, in all caſes of this diſorder, without making any exceptions, the operation of extraction, in preference to that of depreſſion; and I believe it is now generally acknowledged by medical men, that in the more common caſes, his deciſion as to the mode of operating is perfectly well founded. The Baron admits that the operation is not ſo certain a cure in children as it is in perſons of a more advanced age; both on account of their untractableneſs, and becauſe, in them, the opacity of the cryſtalline is not unfrequently accompanied with an opacity in the capſule that contains it. On theſe accounts, when children are born with this diſorder, he adviſes to poſtpone the operation, until they are old enough to be made ſenſible of the loſs they ſuſtain by the want of ſight, and have firmneſs of mind to ſubmit patiently to the means that are requiſite in order to obtain it. Influenced by this opinion of the Baron, and believing the operation of extraction to be ſo much ſuperior to that of depreſſion, that the latter ought not, on any occaſion, to have the preference, I have given advice, in the caſes of a conſiderable number of children who were born with this diſorder, to poſtpone every attempt to relieve them, until they were thirteen or fourteen years old. Prior to this time, it did not appear to me that children could be depended upon to ſubmit, with due ſteadineſs, to the repeated introduction of

 inſtruments,

inſtruments, which is ſometimes neceſſary in extracting the cataract; and, even at this age, the eyes of ſome are ſo ſmall, and in ſuch a conſtant rolling motion, that it is almoſt impoſſible properly to accompliſh the operation. The Portugueſe lad, whoſe caſe has been related, afforded an inſtance of this kind; and I conſider it as a fortunate circumſtance that it came under my notice, ſince, in ſome degree, it may be ſaid to have obliged me to examine, more attentively than I had before done, the advantages and diſadvantages of the operation of depreſſion; which operation, being more eaſy to perform than that of extraction, has certainly this advantage in the caſes of children, (to which alone I here advert,) that it may be performed with equal ſafety when they are only ſeven years of age, as it may at any ſubſequent period of their lives.

Pott favoured the practice of depreſſion.

It is well known that the late Mr. Pott, who publiſhed his remarks on the cataract in the year 1775, was a ſtrenuous advocate for this operation; and, though he appears to me to have much under-rated the advantages of extraction, it muſt be allowed that he makes many juſt and highly pertinent obſervations on the uſe of the couching needle, in thoſe caſes where the cataract is ſoft, or fluid. Mr. Pott conſidered this as a very common ſtate of the diſorder; and does not make any diſtinction between the cataract when it attacks grown perſons, and when children are born with it. In the former caſe, experience inclines me to believe, that the cataract is very rarely fluid, or even ſoft; whereas, in the latter, I have always found it, agreeable to the obſervation of the Baron de Wenzel, in one or other of theſe ſtates. Although, therefore, in the caſe of grown perſons, the operation of extraction appears to me to have very great advantages over that of depreſſion, yet, in the caſe of children, I can readily accede to almoſt the whole that Mr. Pott advances in favour of depreſſion. If the couching needle be paſſed in the way in which it is uſually introduced to depreſs the cataract, and thereby a large aperture be made in the capſule of the cryſtalline, (which operation may be performed with perfect ſafety, and with very little pain to the patient, whilſt the eye is fixed with a ſpeculum oculi,) the opaque cryſtalline, being thus brought into contact with the aqueous and vitreous humours, will, in a ſhorter or longer ſpace of time, according to its degree of ſoft-

Reaſons in ſupport of Pott.

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nefs, be abforbed; and, if there be not an opacity in the capfule, as well as in the cryftalline, the pupil will become clear, and the patient will acquire a very ufeful fight. If, in addition to the opacity of the cryftalline, the capfule be alfo opaque, and, in confequence of this, the operation do not prove fuccefsful, the eye will neverthelefs be perfectly uninjured, and it will be as fit, at a fubfequent period, to have the capfule extracted, as it would have been if no attempt of the above kind had been previoufly made.

From the foregoing obfervations, I flatter myfelf I fhall be juftified in deducing the following inferences.

Thofe born blind from cataracts diftinguifh colours, and approach or recefs; though very imperfectly;

First, When children are born blind, in confequence of having cataracts in their eyes, they are never fo totally deprived of fight as not to be able to diftinguifh colours; and, though they cannot fee the figure of an object, nor even its colour, unlefs it be placed within a very fhort diftance, they neverthelefs can tell whether, when within this diftance, it be brought nearer to, or carried farther from them.

which greatly helps their judgment on acquiring the power of vifion.

Secondly, In confequence of this power, whilft in a ftate of comparative blindnefs, children who have their cataracts removed, are enabled, immediately on the acquifition of fight, to form fome judgment of the diftance, and even of the outline, of thofe ftrongly defined objects with the colour of which they were previoufly acquainted.

Such children may in general be cured in the way of deprefsion.

Thirdly, When children have been born with cataracts, the cryftalline humour has generally, if not always, been found either in a foft or fluid ftate. If, therefore, it be not accompanied with an opacity, either in the anterior or pofterior portion of the capfule, and this capfule be largely punctured with the couching needle, introduced in the way in which this inftrument is ufually employed to deprefs the cataract, there is reafon to expect that the opaque matter will, fooner or later, be abforbed, the pupil become clear, and the fight be reftored.

This procefs does not prevent fubfequent extraction.

Fourthly, If, in addition to the opacity of the cryftalline humour, its capfule be alfo opaque, either in its anterior or pofterior portion, or in both, (which circumftance cannot be afcertained before the operation,) and, in confequence of this, the operation above mentioned fhould not prove fuccefsful, it will not preclude the performance of extraction afterwards, if this be thought advifeable.

Fifthly,

Fifthly, The operation above mentioned being much more easy to perform than that of extraction, and it being possible to fix the eye with perfect safety during its performance, by means of a speculum oculi, it may be undertaken at a much earlier age than the latter operation; and a chance may of course be given to the patient, of receiving instruction, without that loss of time which has usually been thought unavoidable, when children are born with this disorder.

As the depression may be made earlier in life, the subject will gain earlier instruction.

IT ought to be mentioned, that about a month after the above mentioned operation on Master W.'s left eye, I performed a similar operation on the right eye of the same young gentleman. Although he behaved with great firmness on the first occasion, it was not without considerable difficulty that his head was kept steady on the second. The operation, however, gave him very little pain, and no inflammation followed; but the opacity afterwards was not diminished; and he did not acquire any additional sight from this eye. There was an evident mark in that part of the capsule where the couching needle pierced it; though the aperture was too small to admit a sufficient number of rays of light to give an idea of objects. It seems probable that the want of success, in this instance, was owing to an opacity in the capsule, which was incapable of being absorbed. The eye, however, is as fit to have the aperture in the capsule enlarged, or a portion of it removed, when the patient is of a proper age, as if no operation had been previously performed.

The second operation was performed a month after the first. It did not restore the eye.

I beg leave also to add, that since these pages were put together, a case has come under my care, which seems to afford a confirmation of the remarks that have been offered respecting the state of the cataract in children, and the effects that are likely to be produced by the operation of puncturing the capsule that contains it. A young lady, eighteen years old, was put under my care, who had been blind from an early part of her infancy. She had a cataract completely formed in both eyes; and in each there were three or four opaque spots, more white than the rest, which seemed to lie on the surface of the opaque crystalline. I punctured the capsule of each with a couching needle, according to the proposition in the

Narrative of a case which adds force to the preceding inductions.

preceding pages, in the presence of Mr. Scott, surgeon, in St. Alban's-street. The operation gave her no pain; and, in the course of a few days, the opacity was evidently diminished, particularly in the right eye, the patient discovering the colour of objects more plainly than before, but being still unable to distinguish their figure. At the end of a month, finding no further improvement in her vision, it appeared to me most probable that the remaining opacity was situated in the capsules. I therefore determined to extract either a part or the whole of each of them. The incisions of the cornea were made in the usual manner; after which, I punctured the anterior parts of both the capsules with the sharp end of a gold curette. The punctures became immediately transparent, without affording an issue to the liquor Meibomii, or any other humour. From hence it seems evident, that nothing was contained within the capsules, or, in other words, that the crystalline humours were absorbed; and it appears to me highly probable, that their absorption had been occasioned by the previous operation of puncturing their capsules with the couching needle. I dilated the new punctures with the end of the curette; and afterwards, being still afraid that the apertures in the capsules might not be large enough to admit a sufficient number of the rays of light, I removed a portion of each of them with a small forceps. This was accomplished in the left eye, without occasioning the discharge of any part of the vitreous humour; and, in the right, the quantity of this humour that came away was very small. In the course of a week, the inflammation that followed the operation was nearly removed; a large portion of both pupils was quite clear; and the young lady distinguished objects with quickness and precision.

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all these blank ſpaces; that is to ſay, his table comprehends the right aſcenſion and declination of the middle of each of theſe ſpaces.

The changeable ſtars are comprehended in a ſecond table; they are thirty one in number. There are not more than twelve, the periods of which are known; but there are ſeveral others which are diminiſhed ſo as to diſappear. By following them attentively, we may determine the time which elapſes between two ſucceſſive diſappearances; and this is a ſpecies of obſervation which Citizen Lalande propoſes to the curioſity of thoſe who, having only indifferent inſtruments, wiſh nevertheleſs to be uſeful to aſtronomical ſcience.

A third table preſents thirty-three ſtars of a red colour. Since the year 1756, Mayer had remarked this colour in the 19th of Piſces, which he deſignates in his regiſters of obſervations by the epithet of *Rubicunda,* as he finds by the copy, which Prof. Lichtenberg has ſent of all the obſervations made by Mayer, on the day when he obſerved Herſchel's planet. Mitchell and Baillie ſuppoſe, that the colours of the ſtars may depend either on the different intenſity of their fire, or the degree of their inflammation, and that the red colour may indicate a fire which is in a diminiſhing ſtate. According to this hypotheſis, it would be a matter of importance to examine the changes of colour which take place in the ſtars. Be that as it may, theſe variations, if they exiſt, are certainly extremely ſlow; for the different ſhades of colour which are remarked at the preſent day in Antares, Arcturus, Aldebaran, Sirius, and the Lyre, exiſted in the age of Ptolemy.

Coulomb on magnetiſm.

Extract of a memoir on the degree of magnetiſm which blades of ſteel of different thickneſs acquire, and on ſome reſults relative to the needles of the compaſs, by CITIZEN COULOMB.—Almoſt all the magnetic phenomena may be reduced to calculation, if we ſuppoſe two magnetic fluids to exiſt in the ſteel, in each of which the molecules repel each other in the inverſe ratio of the ſquare of the diſtances, and attract the particles of the other fluid in the ſame ratio. This law has been proved by C. Coulomb in the Memoires of the Academy of Sciences for 1786, according to experiments which appear to be deciſive.

When the ſteel is in its natural ſtate, and has not been touched with the load ſtone, the two fluids are neutralized by each other; that is to ſay, they keep themſelves in equilibrio, and

and exert no action, since one of the fluids attracts a magnetic particle with the same force as that with which the other repels it.

When the fluids are separated, they tend to unite and neutralize each other; and they would actually unite, were there not a coercive force that opposed this union. This force may be either the adhesion of the molecules of the fluid to the steel, or the friction which they undergo in passing from one point to another.

The author has proved, *Memoires de l'Academie des Sciences pour* 1787, page 491, that whether we suppose the two fluids to be separated and carried each to one extremity of the blade, or that they are only displaced in each particle of the steel, the result of the calculation will be the same. He has proved at the same time, that this latter supposition is the only one which can be made to agree with the facts.

In the present memoir the author endeavours to determine the magnetic state of several blades united successively one above the other; or, which amounts to the same thing, he endeavours to determine the powers of different blades relative to their thickness.

New Planet.

New planet between Mars and Jupiter.

Mr. Piazzi, of the University of Palermo, discovered on the 1st of January, 1801, a star which appears to be a new planet. By observations repeated for several succeeding days he concluded, that its orbit is not likely to be parabolic, but agrees best with the hypothesis of a circle, the radius of which appears to be 2,6362 of the earth's mean distance, and consequently its position will be between Mars and Jupiter. Its bulk appears to be about $1\frac{1}{3}$ that of the earth. He has assigned to it the name of *Ceres Ferdinandia,* being the names of the ancient divinity of Sicily, and of its present sovereign the founder of the Observatory at Palermo. An account has been lately presented to the Royal Society. At present I give the following particulars from a paper in German.

OBSERVATIONS,

SCIENTIFIC NEWS.

NATIONAL INSTITUTE OF FRANCE.

Extract of the Report of the Tranſactions of the Claſs of Mathematical and Phyſical Sciences, during the laſt Trimeſtre of the Year 9. French National Inſtitute.

Mathematical Part. By CITIZEN DELAMBRE.

Memoir on the Equilibrium of Arches. By CIT. BOSSUT. Boſſut on arches.

AS the key ſtones of which an arch is compoſed, ſupport themſelves by mutual equilibration, and remain ſuſpended without the aid of any ſupport beneath, their whole force directing itſelf againſt the buttreſſes; in order to enſure the duration of an arch, it is not ſufficient that all its key-ſtones be in equilibrio; but it is neceſſary that its ſupports at the ends ſhould oppoſe a reſiſtance adequate to the force which it exerts in order to overthrow or break them in pieces.

The enquiry reſpecting the means proper for preventing their being overthrown, conſtitutes what is termed the problem of the lateral preſſure of arches. Several geometricians of the laſt century had already occupied themſelves with this ſubject, but they had intirely neglected to inquire into the means of preventing theſe ſupports from being cruſhed.

C. Boſſut undertook, in the year 1770, to treat the queſtion in the moſt general manner, as well for ſimple arches as domes. He has inveſtigated whatever relates to the figure and the lateral preſſure of vaults. His memoirs have been printed in the volumes of the Academy of Sciences for the years 1774 and 1776.

After many new obſervations and various experiments, which may be of the greateſt practical utility, C. Boſſut has reſumed the ſubject of his two memoirs. He has new modelled them by ſimplifying his calculations in ſeveral places, and has made a number of additions relative both to theory and practice; ſo that the whole, in its preſent ſtate, conſtitutes a work that may be conſidered as original.

Comets.—C. Meſſier has read an account of the comet which he diſcovered on the 23d of Meſſidor laſt, about half an hour Comet of July 12, 1801.

hour after eleven in the evening: its light was very feeble; in 41 minutes of time it moved 24 min. 40 sec. of right ascension direct, and 6 min. 38 sec. north declination decreasing. The same comet was seen on the same day, and almost at the same instant by Citizens Mechain and Bouvard; the latter of whom had even observed it at the meridian at 11^h 57^m 49 sec. true time. The right ascension was 111 deg. 15 min. and the north declination 69 deg. 30 min.

C. Pons observed it on the same day at Marseilles, and he had even perceived it on the preceding day; but the clouds did not at that time permit him to ascertain, by regular observations, whether it was a comet, or merely a nubecula.

Summer solstice, 1801.

Observation of the summer solstice of the year 9.—Citizen Duc-la-Chapelle, associate, has communicated to us the result of the observations, made by him at Montauban, in order to determine the solstitial height of the sun, and the obliquity of the ecliptic.

From an average of nine days observations, he finds 23 deg. 28 min. 9 sec. for the apparent obliquity, taking 15 min. 48 deg. for the semi-diameter of the sun, and 44 deg. 0 min. 52 sec. for the latitude of his observatory.

Lalande on the fixed stars.

Remarks concerning the 50,000 *stars, of which observations have been published by Citizen Jerom Lalande.*—C. Lalande, in the preface to his *Histoire Céleste,* had noticed the existence of many blank spaces in the heavens, many changeable stars, and many red stars. He now resumes these subjects in a more circumstantial manner, in a memoir accompanied with tables.

By blank spaces, (*d'espéces vuides*) Citizen Lalande understands in this memoir, those spaces in which no stars of the 9th degree of magnitude are to be perceived. These are the smallest that can easily be distinguished with an achromatic telescope of 67 millimeters aperture, in which light is admitted to illuminate the wires.

It is not to be doubted that by excluding all light from surrounding objects, and employing the most powerful telescopes, the blanks properly so called would be found to be considerably diminished; perhaps even there is not a single spot in the firmament, towards which we could direct a telescope, without perceiving a great number of stars, though below the ninth degree of magnitude, and consequently too small to be of any use in astronomy. C. Lalande gives the catalogue of

4 all

A Treatiſe on Aſtronomy, in which the Elements of the Science are deduced in a natural Order, from the Appearances of the Heavens, to an Obſerver on the Earth; demonſtrated on Mathematical Principles, and explained by an Application to the various Phenomena. By Olinthus Gregory, *Teacher of Mathematics, Cambridge.* 8vo. 522 Pages, with Nine Plates. London, ſold by Kearſley. Gregory's Aſtronomy.

Elements of Chemiſtry. By J. Murray, *Lecturer on Chemiſtry, Materia Medica, and Pharmacy.* 8vo. 2 vols. 692 pages. Edinburgh printed. Sold by Longman and Rees, London. Murray's Chemiſtry.

Obſervations on the Winds and Monſoons, illuſtrated with a Chart, and accompanied with Notes, Geographical and Meteorological. By James Capper, *formerly Colonel, and Comptroller-General of the Army and Fortification Accounts on the Coaſt of Coromandel.* Quarto. 234 Pages. Debrett. Capper on Winds and Monſoons.

Accounts of the three laſt mentioned Works will appear in our next.

Uber den Werth und Nutzen der Runkelruben.—On the Value and Uſe of the White Beet-Root. By J. G. W. 1801. 29 Pages. Glogau. J. G. W. on Beet-ſugar.

This pamphlet contains minute inſtructions for preparing ſugar, ſyrup, coffee, brandy, rum, and arrack, from the white beet-root (beta cicla). In order to obtain ſugar from the beet-root, the author directs the roots to be cut into ſlices, to expreſs the juice, to boil it down to a ſtrong conſiſtence, and to ſuffer it to cryſtallize. The reſidue, from which the juice has been expreſſed, may be uſed for obtaining brandy, but it is better to boil the beet-root to reduce it to a pulp, ſuffer it to ferment, and then ſubmit it to diſtillation. In order to deprive the ſpirit of the peculiar flavour of the root, it muſt be rectified over charcoal powder. Eighty pounds of beet-root yielded eight quarts of brandy. To obtain rum, the roots are likewiſe boiled, the juice expreſſed, and mixed with charcoal powder, evaporated to one-third, then ſuffered to foment, and afterwards diſtilled with the charcoal powder. In order to obtain arrack, the juice is to be evaporated to one-half, then ſuffered to ferment, and diſtilled.

Recherches

Des-Mortiers on Prussian Blue.

Recherches sur la Décoloration spontanée du Bleu de Prusse, &c.—Inquiries into the spontaneous Loss of Colour in Prussian Blue, and the spontaneous Return of its Colour; read at a private meeting of the Free Society of Sciences, Literature, and Arts, at Paris, by U. R. T. le Bowyer-des-Mortiers, *Member of the Society.* Paris. 8vo. 32 p. 1801.

A person had made for sale a pretty considerable quantity of blue paint, consisting of a mixture of white lead, Prussian blue, and nut oil. To prevent its drying he covered it with water some inches deep, and set it by till wanted. After a certain time, a person coming to buy some of the paint, he was much surprised to find it all white, except at the surface, where the colour was well preserved. He was preparing to add fresh Prussian blue to the paint, when, on grinding it in the open air, without any addition, he saw the colour return of itself, and on continuing this operation, it became as deep a blue as at first. He then covered it with oil, supposing it would keep better than under water; but he was disappointed, for the colour disappeared a second time throughout the whole mass.

The Society of Physic and the Arts at Nantes, to whom some of this paint was carried in its white state, spread part of it on writing paper, part on wood, and part on the wall of a window. After a longer or shorter time, the colour of the paint re-appeared with all its lustre, but that on the paper was most slowly restored.

What is the cause of this phenomena? Is it the oil, which, by undergoing a change, destroys the colour of the Prussian blue? Is it the air altogether, or one of its constituent principles, or any other substance mixed or dissolved in it, that restores the colour? These are the questions which the author endeavours to answer, in publishing the different interesting experiments he has made on the subject, from which he deduces the following conclusions.

1. The loss of colour in the paint is not owing to the decomposition of the oil, but to a change of surfaces, occasioned by the subsiding of the mass, and by the extinction of the luminous globules in the minute laminæ and in the pores of the colouring substance. 2. Neither the air altogether, nor one of its constituent principles, nor any thing it contains, is necessary

2

OBSERVATIONS.

1801	Med. Time. h m	Geo. Long. ° ′	Geo. Lat. ° ′	Right As. ° ′ ″	Decl. ° ′ ″
June 20	13. 4	101. 45	3. 26 N	103. 6 40	26 22 10 N
July 17	1. 43	113. 3	4. 6	115. 38 50	25 32 35
Aug. 12	10. 54	124. 21	4. 51	127. 56 5	23 54 10
Sept. 7	16. 19	135. 23	5. 41	139. 44 30	21 38 25
12	22. 0	137. 40	5. 52	142. 2 20	21 8 10
18	3. 0	139. 50	6. 3	144. 17 0	20 37 0
23	8. 0	141. 58	6. 15	146. 29 20	20 6 25
28	13. 0	144. 5	6. 27	148. 40 0	19 35 0
Oct. 3	17. 41	146. 9	6. 40	150. 47 0	19 4 40
8	22. 0	148. 12	6. 53	152. 50 50	18 33 40
14	3. 0	150. 12	7. 8	154. 55 30	18 4 40
19	7. 0	152. 11	7. 22	156. 56 20	17 34 20
24	11. 0	154. 8	7. 37	158. 55 0	17 5 10
29	14. 45	156. 3	7. 53	160. 51 30	16 37 0
Nov. 3	18. 0	157. 56	8. 9	162. 45 40	16 9 0
8	22. 0	159. 48	8. 26	164. 38 50	15 41 50

	S ° ′ ″
Place of Afcending Node - - - - -	2. 20. 58. 30
Inclination of the Orbit - - - - - - -	10. 47. 0
Place of the Aphelion - - - - - - -	2. 8. 59. 37
Time of paffage through the Aphelion Jan. 1. 1801 - - - - - - - - - - - -	1,3328
Excentricity - - - - - - - - - -	0,0364
Log. of the half great Axis - - - - -	0,4106986
Time of the Siderial period - - - - -	4,13 years.

ACCOUNT OF BOOKS OF SCIENCE.

Philofophical Tranfactions of the Royal Society of London, for the Year 1801. *Part the Second. Quarto,* 214 *Pages, with Eighteen Plates.* London, fold by Elmfly.

THIS Part contains :—1. A Hiftorical and Anatomical Defcription of a doubtful Amphibious Animal of Germany, called by Laurenti, Proteus Anguinus. By Charles Schreibers, M. D. of Vienna.—2. Obfervations tending to inveftigate the Nature of the Sun, in order to find the Caufes or Symptoms of Royal Society of London.

Royal Society of London.

of its variable Emiſſion of Light and Heat; with Remarks on the Uſe that may poſſibly be drawn from Solar Obſervations. By William Herſchel, LL. D. F. R. S.—3. Obſervations on the Structure and Mode of Growth of the Grinding Teeth of the Wild Boar, and Animal Incognitum. By Everard Home, Eſq. F. R. S.—4. Account of ſome Experiments on the Aſcent of the Sap in Trees. By Thomas Andrew Knight, Eſq.—5. Additional Obſervations tending to inveſtigate the Symptoms of the variable Emiſſion of the Light and Heat of the Sun; with Trials to ſet aſide Darkening Glaſſes by tranſmitting the Solar Rays through Liquids; and a few Remarks to remove Objections that might be made againſt ſome of the Arguments contained in the former Paper. By William Herſchel, LL. D. F. R. S.—6. On an improved Reflecting Circle. By Joſeph de Mendoza Rios, Eſq. F. R. S.—7. Obſervations and Experiments upon Dr. James's Powder; with a Method of preparing, in the Humid Way, a ſimilar Subſtance. By Richard Chenevix, Eſq. F. R. S. M. R. I. A.—8. Caſe of a young Gentleman, who recovered his Sight when ſeven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks. By Mr. James Ware, Surgeon.—9. An Account of ſome Galvanic Combinations, formed by the Arrangement of ſingle Metallic Plates and Fluids, analogous to the new Galvanic Apparatus of Mr. Volta. By Mr. Humphry Davy, Lecturer on Chemiſtry in the Royal Inſtitution.—10. A Continuation of the Experiments and Obſervations on the Light which is ſpontaneouſly emitted from various Bodies; with ſome Experiments and Obſervations on Solar Light, when imbibed by Canton's Phoſphorus. By Nathaniel Hulme, M. D. F. R. S. & A. S.—11. Experiments on the Chemical Production and Agency of Electricity. By William Hyde Wollaſton, M. D. F. R. S.—12. Farther Obſervations on the Effects which take place from the Deſtruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafneſs. By Mr. Aſtley Cooper.

A Treatiſe

ceffary to reftore the colour; for it revives *in vacuo* as well as in the open air. 3. Caloric, without the contact of light, is detrimental to the return of the colour, inftead of contributing to it, and even deftroys it. 4. Finally, the inteftine motion of its parts, however exerted, is fufficient to re-produce the colour more or lefs fpeedily, according as there are more or lefs light and motion.

Effai fur le Calorique.—An Effay on Caloric; or, Inquiries into the Phyfical and Chemical Caufes of the Phenomena exhibited by Bodies fubjected to the Action of the Igneous Fluid, with new Applications of them refpecting the Theory of Refpiration, Animal Heat, the Origin of Volcanic Fires, &c.; to which are added, An Effay on the Anomalies of Chemical Affinities; Experiments and Obfervations on Bell-metal; and a Defcription of the famous Alum Mine of Souvignaco, in Iftria, and of the Proceffes employed for extracting and purifying the native Alum; by Jofeph-Mary Socquet, M. D. *formerly Phyfician to the Army.* Paris. 8vo. 473 p. Price 5fr. 1801. **Socquet on Caloric.**

Natural philofophers are yet far from being agreed on the nature of caloric, and the phenomena afcribed to it. In this volume Dr. S. has publifhed eight effays on the fubject.

The 1ft treats of caloric confidered in its chemical and phyfical relations to other bodies, whence may be deduced the principal phenomena they exhibit when fubjected to the action of this fluid; as their capacity for caloric, their dilatation, fufion, gafification, tendency to equilibrium of temperature, &c.

In the 2d the author turns his attention to the caufe of the perpetual production of heat by the friction of bodies, and of the confequences which Count Rumford deduced from his own experiments on the fubject.

In the 3d he treats of the conducting power of liquids for caloric.

The 4th contains new views on refpiration, and the caufe of the production of heat in warm-blooded animals.

The fubject of the 5th is the nature of volcanic fires, the caufes of which the author deems independent of any local combuftion, produced by the decompofition of water or of any other oxide, or by fubmarine or fubterranean currents of air.

The

Socquet on Caloric.

The 6th contains feven cafes of chemical affinity:—1. Predifpofing affinities, as they are called: 2. The decompofition of fulphate of iron by cauftic ammoniac: 3. The decompofition of muriate of foda by fulphate of magnefia, or of the zero of Reaumur: 4. The reciprocal decompofition of alkaline phofphates by carbon, and of alkaline carbonates by phofphorus: 5. The decompofition of water in the folution of iron by diluted fulphuric acid; and the decompofition of the acid, not of the water, in diffolving quickfilver by the fame menftruum: 5. The precipitation of free fulphuric acid, by mixing the two fulphates of mercury and ammoniac: 7. Whence comes it, that thofe portions of folvents which are laft fixed in their bafes eafily feparate from them, while the firft portions, though of the fame nature, and fixed in the fame fubftances, adhere to them much more forcibly?

The author's refidence in Italy having given him an opportunity of travelling over almoft all the provinces of the quondam ftate of Venice, and fome other countries dependent on Auftria, he vifited a great number of productive and interefting manufactories. The extenfive alum mine of Souvignaco, in Iftria, of which there was no defcription extant, probably on account of its very remote and wild fituation, and perhaps becaufe it is of no ancient date, afforded him fome curious particulars refpecting the difpofition and nature of the ore, the fimple, ingenious, and partly new proceffes followed there, and the theory of the various phenomena they exhibit. Thefe are the fubjects of the 7th effay.

In the 8th Dr. S. gives a fummary of his experiments on the extraction of pure copper from bell-metal, the refults of which had already been publifhed in feveral Italian journals; together with fome other operations in the large way, which he had executed at Venice on different objects of art, particularly the feparation of foda from common falt *. The corrections, or rather additions, propofed by the Chev. Napione to the proceffes already known for decompofing the different kinds of bronze, were too interefting, for our author to omit making them known at large; particularly as he had an opportunity of convincing himfelf of their utility, by repeating the experiments of that learned mineralogift, at Turin.

* See *Opufcoli fcelti fulle Scienze e fulle Arti: da Carlo Amoreti: tom.* XX. Milan.

Atlas

Atlas de la Partie Meridionale de l'Europe, &c.—An Atlas of the Southern Part of Europe, constructed to the Meridian of Paris, and consisting of forty Sheets, which, being on the same Scale, may be pasted together, so as to form the most comprehensive Map that has ever yet been published of the different States it includes: by C. Chanlaire, *Member of the Lyceum, and one of the Authors of the National Atlas of France.* Paris. 1801.

Chanlaire's Atlas of the South of Europe.

This map is remarkable for the accuracy of its details, and may supply the place of a great many maps of single countries.

Mémoires d'Agriculture, &c.—Memoirs of Agriculture, and Rural and Domestic Economy, published by the Agricultural Society of the department of the Seine, and printed by order of the Prefect of the department. Vol. I. Paris. 8vo. 447 p. 1 plate. Price 4fr.

Memoirs of the Agricultural Society of the department of the Seine.

An account of this work will appear in our next.

BOOKS OF SCIENCE,

Imported by Deboffe, of Gerrard-Street, Soho.

ANNUARE de la Republique Francoise, par le Bureau des Longitudes, pour l'An ix. Par. An. viii. in 18. br. 1s. 3d.

Books of science imported.

Carnot de la Correlation des Figures de Geometrie. Par. 1801. 8vo. Fig. br. 7s.

Clairault Elém. d'Algebre. 6th Edit. 2 Vol. 8vo. Par. 1801. Fig. br. 14s.

Essais sur la Ligne Droite et les Courbes du Second Degré par Francois. 8vo. Par. 1801. 4s.

Gallitzin Recueil de Noms, &c. appropriés en Mineralogie suivi d'un Tableau Mythologique. Brunswick, 1801. 4to. br. 12s.

Haüy Traité de Mineralogie publié par le Conseil des Mines. Par. 1801. 4 Vol. avec Atlas. 2l. 2s.

Memoires

Books of ſcience imported.

Memoires de l'Inſtitut National, 3e livraiſon An. ix. 3 Vol. 4to. Fig. cart. 3l. 3s.

Memoires des Societes ſavantes de la Republique Francoiſe, par Prony, Parmentier, Duhamel, &c. 4to. pour l'Année. 1l. 16s.

Monge, Traite Elementaire de Statiſtique Par. An. ix. 8vo. Fig. br. 6s.

Olivarius le Nord, Litteraire, Phyſique, Politique et Morale Ouvrage periodique publié tous les 3 Mois, les 16 Premiers Cahiers. 8vo. br. 2l.

O'Reily Eſſai ſur le Blanchiment. Par. 1801. 8vo. Fig. br. 7s. 6d.

Pallas Obſervations faites dans un Voyage enterpris dans les Gouvernemens Meridionaux de l'Empire de Ruſſie, pendant les Années 1793 & 1794. Leipz. 1801. Tom. 2. 4to. Atlas. 4l. 14s. 6d.

Philoſophie de Kaunt, ou Principes fondamentaux de la Philoſophie tranſcendentale, par Villiers. Metz. 801. 8vo. br. 6s.

Plans & Deſſins tirés de la Belle Architecture, ou Repreſentations d'Edifices executés on projettés, en 115 Planches par Stieglitz. Londr. 1801. Fol. Cart. 9l. 9s.

Principes d'Economie Politique: Ouvrage couronné par l'Inſtitut Nationale, par Canard. Par. 1801. 3s.

Traite Elementaire de Phyſique par Libes, Profeſſeur aux Ecoles Centrales de Paris. Par. 1801. 3 Vol. 8vo. Fig. br. 1l. 4s.

Voyage dans l'Inde et au Bengal, pendant les Années 1789 et 1790, par Grandpre. Par. 1801. 2 Vol. 8vo. Fig. br. 14s.

Voyage en Syrie, No. 20. Proof Plates, 2l. 12s. 6d.—Not Proof, 1l. 15s.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1802.

ARTICLE I.

On the ſuppoſed Currents in hot Liquids. *By* THOMAS THOMSON, M. D. *Lecturer on Chemiſtry in Edinburgh.* *(From the Author.)*

THERE is perhaps no ſubject of enquiry in which we are more liable to deceive ourſelves than in the inveſtigation of the internal motions of the particles of bodies. As theſe particles are individually too minute to come under the cogniſance of our ſenſes, their motions muſt of courſe be inviſible, and can only be inferred from certain viſible changes which we conſider as the conſequences, or at leaſt as the conſtant attendants of theſe motions. But unfortunately it ſeldom happens that the phenomena, from which we draw our inferences, are of ſuch a nature as to entitle us to affirm that they can only originate from one ſingle cauſe, and from no other. They may be aſſigned in general to a variety of cauſes; and much patient induſtry, with no ſmall ſhare of addreſs, is neceſſary to develope the true one; and even all the precautions which we can employ do not always prevent us from falling into error. The phenomenon, which forms the ſubject of this paper, affords a very ſtriking illuſtration of the great caution with which we ought to draw concluſions, even when the inference ſeems at firſt ſight to follow irreſiſtibly from the phenomena.

Great difficulty of inveſtigating the internal motions of the particles of bodies.

Concife narrative of Count Rumford's experiments to render currents obfervable in fluids; by floating grains of amber.

Count Rumford was firft led to fufpect that fluids are non-conductors of caloric, by the appearance of certain opaque particles moving upwards and downwards in them, while they were cooling in a window, expofed to the direct light of the fun. He very naturally concluded that thefe motions were occafioned by the currents of the liquid moving in the fame direction with the opaque bodies. Hence he inferred that every individual particle of the liquid was cooled only by depofiting its caloric at the furface or the fides of the veffel, and that therefore thefe particles were incapable of giving out heat to each other. The contrivance by which he rendered thefe motions vifible to the naked eye was in the higheft degree fimple and ingenious. He mixed amber, in the ftate of fmall grains, with an alkaline folution, diluted with water till its fpecific gravity was exactly the fame with that of amber. The amber, of courfe, remained fufpended in any part of the liquid; but as foon as heat was applied, it began to move upwards and downwards, exhibiting the currents to the eye of the fpectator.

The motions of thefe grains have always been thought demonftrative of the exiftence of currents;

Nothing can appear a more fatisfactory proof of the exiftence of currents in the liquid than the motion of the amber; fo convincing is it indeed, that hitherto no perfon, as far as I know, has ever fufpected the poffibility of this motion originating from any other caufe. I have repeated the experiment frequently, and even exhibited it to others; and though the motions did not exactly correfpond with thofe indicated by Count Rumford, they never failed to produce the fulleft conviction of the exiftence of the very currents which that philofopher had indicated. Yet the fact is, that the motion of the amber is not occafioned by currents in the liquid, and very often exifts when there are really no currents at all. * This affertion will probably, at firft fight, appear rather extraordinary; but the proofs on which it is founded leave no room for doubt.

but they are not moved by currents.

Difficulties attending the fuppofition of currents as rapid as the motions of the amber.

When we reflect upon thefe fuppofed currents, feveral circumftances cannot fail to prefent themfelves, which do not admit of an eafy explanation. The motion of the amber is pretty rapid, yet the mere difference in the fpecific gravity of

* I neglect the trifling currents occafioned by the amber itfelf, which it would be extremely difficult to appreciate.

the

the hot and cold particles of the liquid, the causes assigned by Count Rumford for the existence of the supposed cnrrents, is capable of producing only a motion so very slow, as scarcely to be visible. But what is still more puzzling, if possible, is the total discordance between these rapid currents and the slow rate at which the liquid cools. If the water actually moved with the rapidity indicated by the amber, and if it parted with the tenth part of the caloric necessary to produce that rapidity, the whole of the liquid would cool down to the temperature of the atmosphere almost in an instant: whereas, in fact, several hours are always necessary to produce that effect.

Experiment 1. Upon water, tinged blue, was poured clear water with such care as not to mix them. Several opaque bodies floated in these fluids.

These apparent anomalies suggested the possibility of the motion of the amber being occasioned by some other cause than currents in the liquid, and made it necessary to examine the matter with more attention. The following experiments occurred to me as the simplest method of determining whether the supposed currents existed or not. Into the glass vessel A, Pl. VIII. Fig. 2, I poured water, tinged blue with the juice of red cabbage, till it stood at the height *m*. Then by means of a slip of cork swimming on the surface of the blue infusion, and a funnel terminating in a long capillary tube, I succeeded (after several unsuccessful attempts) in filling the vessel with water to the height *n*, without mixing it with the blue infusion. Thus I had two liquids in the vessel A, both very nearly of the same specific gravity, the undermost of a blue colour, the uppermost colourless, and the plane *m*, which separated them, pretty well defined. There were several opaque bodies floating through the blue infusion, which answered the purpose of the amber.

It was expected, when heat should be applied, *either that* the lower fluid would ascend *in a blue current* and carry up the opaque bodies, *or that* these bodies would ascend alone through both fluids, and consequently *without the mediation of any current.*

My intention was to apply heat to the bottom of the vessel A, which I knew would produce the supposed currents, that is to say, would set the floating bodies in motion. Now if these floating bodies were really put in motion by currents, it is evident that part of the blue infusion must accompany them. But this would become evident whenever they passed the plane *m*, and entered into the colourless medium: for in that case they would be accompanied by a blue current, which would be visible to the eye. Whereas if the floating bodies were in motion while the water continued at rest, they would pass the plane *m*, and move through the colourless water

without

without carrying any of the blue infufion along with them. I accordingly lighted a lamp, and put it at fome diftance below the veffel A, placing the veffel exactly between my eye and the window. The lamp was at fuch a diftance, that it was fome time before the floating bodies began to move. At laft, however, they afcended gradually, paffed the plane *m*, and entered the colourlefs medium, *without being accompanied by any of the blue infufion.* They then defcended again in the ufual manner; and this alternate motion continued for fome time, without occafioning a mixture of the two liquids. Thus I obtained a full demonftration that the motion of the floating bodies was not occafioned by currents in the liquid, but was altogether independent of any fuch currents. Every time that a floating body paffed the line *m*, the blue infufion was agitated, and its furface raifed into a wave. By the conftant repetition of this undulation, the plane *m* became ill-defined, and gradually afcended higher and higher, till at laft almoft the whole of the water acquired a blue tinge. But it was almoft ten minutes before this happened, and the motion of the floating particles, which continued without interruption during the whole time, was altogether unequivocal.

On trial the bodies rofe and circulated through both fluids without any indication of a current; and ten minutes elapfed before the motion had carried the blue tinge through the upper fluid.

Experiment 2: An alkaline folution, with floating grains of amber, was boiled, and left to cool to 150°. The upper part, to the depth of half an inch, was then tinged. No change whatever took place in the tinged fluid, though the amber circulated through the whole fluid, while cooling.

It was ftill neceffary to repeat the experiment with the amber itfelf, in order to afcertain whether or not its motions were of the fame nature with that of the floating bodies in the laft experiment. I therefore filled the glafs veffel A, to the height *n*, with an alkaline folution containing amber floating in it. I then heated the folution boiling hot by holding the veffel over a fire, and even allowed it to boil for fome time. The veffel was then fufpended in a room, where the temperature of the air was 50°, and the alkaline folution allowed to cool down to 150°.* I then tinged the furface of the folution by means of a drop or two of the infufion of red cabbage, fo as to form a coloured ftratum of about half an inch thick at the furface of the folution.† The cooling continued without interruption, and the liquid reached the temperature of the air without the fmalleft alteration in the coloured ftratum, or any mixture of it with the colourlefs folution

* By previous trials I had afcertained that at a higher temperature the experiment does not fucceed.

† The colour thus communicated is yellow when only a little cabbage juice is ufed; too much gives a green colour.

solution below. Yet, during the first part of the cooling, the amber was moving upwards and downwards; particles of it often come to the very surface, and descended again to the bottom. This experiment proves decisively that the motion of the amber is not occasioned by currents, and that there were, in reality, no currents at all during the whole time of cooling from 150° to 50°.

Having thus proved that the motion of the amber is not owing to currents, but that it forces its way through the liquid; it becomes a point of some consequence to ascertain, if possible, the cause of this motion. Perhaps the following observations will contribute to throw some light on the subject.

Why does the amber thus move *through the fluid?*

1. When an alkaline solution containing amber, as exactly as possible of the same specific gravity with itself, is heated in a glass vessel nearly to the boiling temperature, and then suspended in the air to cool, the whole of the amber sinks to the bottom. Hence we see that, though the amber is of the same specific gravity with the cold solution, it is specifically heavier than the same solution when hot. Consequently amber does not expand so much when heated as the alkaline solution does.

Observation 1. Amber expands less by boiling heat than the alkaline solution.

2. But the amber does not remain long at the bottom; the grains of it gradually rise one after another, and ascend to the surface, but with very different velocities, some rapidly, others slowly. After touching the surface they always fall down again to the bottom. These alternate motions continue for some time; but the number of ascending and descending particles gradually diminishes, and at last they all settle at the bottom. It is no uncommon thing for two particles of amber, one ascending, the other descending, to meet together in some part of the solution. In that case they sometimes exchange motions; the one that was formerly ascending now sinking to the bottom, while the descending particle returns again to the surface. If the experiment be made with a transparent solution, and in a good light, every ascending particle of amber will be observed to have an air bubble attached to it which buoys it up. This bubble separates at the surface of the liquid, and the amber, thus deprived of its buoy, tumbles down again to the bottom. When two particles meet, the air bubble sometimes passes from the one to the

Obs. 2. Each particle of amber is carried up by an adherent air-bubble, which it loses at the surface, and then falls again.

the other, which occaſions their reciprocal change of motion* Theſe air bubbles are of different ſizes, hence the different velocities of the pieces of amber.

Obs. 3. The amber acquires a higher temperature by boiling than the ſolution itſelf, and conſequently it generates ſteam.

3. If the alkaline ſolution be made to boil for a few minutes in a glaſs veſſel, and then ſuſpended immediately in the air to cool, bubbles of ſteam continue for ſome time to detach themſelves in great abundance from the pieces of amber, and occaſion a very briſk motion in theſe pieces. Hence we ſee that the amber acquires a temperature higher than 212°, and that it gradually parts with this exceſs of caloric to the ſurrounding liquid.

Obs. 4. Irregular motions after the ſteam has ceaſed; followed by ſubſidence.

4. After all extrication of ſteam ceaſes, the amber continues to move rapidly upwards and downwards in a very irregular manner. The rapidity of theſe motions gradually diminiſhes, and when the ſolution has cooled down to about 140° (the air being 50°) the amber ſettles at the bottom. * Theſe motions are too rapid to be occaſioned by the cooling of the liquid, yet they are certainly connected with its high temperature. I would have ſuppoſed that they indicated the currents into which the liquid was thrown by boiling, as they are extremely ſimilar to what takes place when the liquid is agitated violently, were it not that they laſt much longer, and that they always ſtop when the difference between the temperature of the liquid and of the air is about 90°.

Obs. 5. Aſcent of the amber; whence it is inferred that it cools leſs rapidly than the fluid.

5. After the amber ſettles at the bottom, the cooling goes on without any thing remarkable till the liquid reaches the temperature of about 100° (the air being at 50°). At that temperature ſome of the amber may be obſerved ſuſpended juſt above the bottom of the veſſel; and, as the cooling advances, the amber gradually riſes higher and higher. No motion can indeed be perceived, but if the ſituation of any individual particle be obſerved at intervals, it will appear evident that it has changed its place, and aſcended nearer the

* This experiment requires that the amber be in the ſtate of a pretty fine ſand, otherwiſe it ſettles at the bottom at the very firſt. But even then the currents may be diſtinguiſhed by the naked eye, if the glaſs veſſel be tranſparent. This fact is curious, and can only be explained by ſuppoſing a greater interval than uſual between the currents and the reſt of the water, or by ſuppoſing their ſpecific gravity to be different.

ſurface,

surface. By the time the liquid has reached the temperature of the air, some of the amber may be observed floating on its surface, some of it a little below the surface, and, in short, in almost every part of the liquid. After this the liquid may be left at rest as long as you please; but the amber does not change its situation, unless some very considerable change takes place in the temperature of the surrounding air. Here we see that the water cools down more rapidly than the amber; so that at a certain period the amber becomes specifically lighter than the water, and therefore rises; extremely slowly indeed, because the difference between the specific gravity of the two bodies is small. I supposed at first that the rise of the amber was owing to the descent of the superior strata of the liquid, which, of course, would have forced up the strata in which the amber was; but I was mistaken. For when the surface of the solution was tinged yellow, the yellow stratum remained neatly defined, and without changing its place during the whole of the process.

This ascent is not caused by a current.

Annotation. Atwood, in his Treatise on Motion, Sect. V. Prop. 12, demonstrates that a sphere of evanescent weight will ascend in any dense fluid uniformly with a velocity equal to what it would have acquired by falling in free space through four third parts of its diameter. This is the greatest possible velocity of ascent of a sphere in any fluid. It may seem absurd to attempt a calculation, one of the assumptions of which shall be the diameter of an integrant particle; but if we take any gross quantity (such, for example, as the millioneth part of an inch), for the diameter of a particle of water at its utmost expansion by heat, and deduce its velocity of ascent (which in that case could scarce amount to the two hundredth part of an inch in a minute), we shall find little reason to admit that ascending currents can be produced by the mere enlargement of the particles heated only by contact with the containing vessel. This, however, must necessarily be supposed if the particles be non-conductors with regard to each other.

Ascent of a sphere in a fluid;

Motion of the particles of water by mere expansion must be extremely slow.

The excellent paper before us directs our attention to the mechanical cause of that circulation, which undoubtedly takes place during the communication of heat to fluids from below, and

Circulation of fluids by change of temperature.

and is the principal reaſon of their much ſpeedier augmentation of temperature in this way. The extrication and aſcent of elaſtic fluid appears, in moſt and probably in all caſes, to precede, and at laſt to produce circulation in denſe fluids. And on this object there is ſtill an extenſive field for reſearch, concerning the manner in which heat is communicated through fluids. But it would be by no means fair to anticipate, by crude conjecture, thoſe explanations which we may expect to obtain from the experimental reſearch of others. W. N.

II.

A Memoir on the Art of making Gun-Flints. By CITIZEN DOLOMIEU.*

No information to be found in books on the art of making gun-flints.

THE art of making gun-flints, which has been long practiſed in a ſmall territory ſituated between the two neighbouring departments of Loir-Cher and L'Indre, is directed to an object, of which the whole value, commercially conſidered, is of little magnitude; but which is of the firſt neceſſity in the uſe of fire-arms of every deſcription. The author of the preſent Memoir was deſirous of acquiring ſome information reſpecting it, but found that it was to no purpoſe that he extended his reſearches to a variety of works on mineralogy and different ſubjects of the arts and trade. The Encyclopédie is ſilent on this proceſs; excepting in the repetition of a ridiculous prejudice concerning the re-production of flints in places whence they were excavated. From the preciſion with which the figure is given to theſe ſtones, it has even been ſuſpected that they could hardly be afforded at the low price they bear, if in its firſt ſtate the material were not even ſoft. But the art is extremely ſimple in its proceſs; it requires a very ſmall number of inſtruments, a ſhort apprenticeſhip, and a very moderate degree of ſkill to form, by mere fracture, figures ſo accurate, faces ſo ſmooth, outlines ſo direct, and angles ſo neat, that the ſtone ſeems as if cut on the wheel of the lapidary. Five or ſix ſmall blows of the hammer, during one minute of time, are ſufficient to produce

* From Memoires de l'Inſtitute Nationale, III. 348, in part abridged.

the

the ſame perfection of figure as would require more than an hour's labour, if the ſections were to be made by grinding againſt harder ſubſtances, or friction with emery. Leſs than a farthing will pay for a gun-flint from the hand of the workman, but fifty times that ſum would be inſufficient for its purchaſe if it were faſhioned by any other proceſs.

The author regularly proceeds to examine firſt the material beſt adapted to the uſe in queſtion, the inſtruments employed, and the manipulation by which the ſtones are faſhioned. Order of deſcription.

With regard to the material, every kind of ſtone, capable of producing ſtrong ſparks when ſtruck againſt ſteel, may be uſed as a gun-flint, if it can but be faſhioned by ſimple and cheap means. But even in this caſe there are ſome motives of preference; ſuch, for example, as that the ſcintillation ſhould be produced with the leaſt poſſible blow, and with no conſiderable wear or abration of the ſteel. Theſe reaſons of predilection are in favour of the ſiliceous ſtones, when compared with thoſe which are called quartzoſe. But the ſilex or flint, properly ſo called, poſſeſſes not only this kind of ſuperiority, but another property, that it is more particularly ſuſceptible of being broken into fragments or plates, which require but very little labour to give them the requiſite form and dimenſions. The material or ſtone moſt fit.

Among the ſilex it is therefore that the fabrication of gun-flints have found the material truly proper for the exerciſe of the art. And among the numerous varieties of this ſpecies of ſtone, there is only one which can be advantageouſly faſhioned by the hammer alone. The agates and chalcedonies, which are alſo applied to this uſe, are brought to the requiſite form by the mill of the lapidary. The makers of gun-flints in France denominate the ſtone of which they make uſe *caillou*, and they themſelves are called *caillouteurs*. The word *caillou* is uſed by them to denote the beſt and moſt ſerviceable kind of flint; whereas, in the other parts of France, it denotes a pebble; that is to ſay, a rounded ſtone, whatever may be its nature. Silex or flint preferred to quartz, becauſe eaſily faſhioned.

The flint of the workmen in gun-flints belongs to that ſpecies of ſilex which naturaliſts have denominated ſilex gregarius, ſilex ignarius, or the feuerſtein of the Germans, &c. But every coarſe flint is not proper for this uſe. In fact, the beſt ſtone is far from being plentiful in nature. Many countries The true *ſilex ignarius* is ſcarce.

tries are entirely deprived of them; and the author thinks that it may probably be affirmed, that France almoſt alone poſſeſſes that variety of ſilex which can be eaſily broken into gun-flints, ſince he cannot ſuppoſe that the art of making them could remain a myſtery to other nations who do not practice it, though they make great uſe of the flints: the art itſelf being ſo ſimple, that they muſt have ſpeedily acquired it, if they have poſſeſſed the material.

Denomination. In his deſcription of the variety of ſilex here alluded to, he gives it the name of ſilex pyromachus to expreſs its uſe, which he prefers to the term ſilex ſclopetarius, as being more muſical.

The external characters are:

External characters. The ſilex pyromachus, when dug up, is always covered with a white external cruſt, one or two lines or more in thickneſs, of an earthy, chalky appearance, and looſe texture, much ſofter and leſs heavy than the ſilex it envelopes. The external form of the maſſes of good ſtones of this deſcription has a ſomewhat convex ſurface, approaching to the globular figure. Thoſe of irregular forms are full of imperfections. The beſt ſtones are not very large. They ſeldom exceed the weight of twenty pounds, and they ought not to be of leſs weight than one or two pounds. Their aſpect, when broken, is greaſy, ſhining a little, and the grain is ſo fine, that it is imperceptible. The colour of theſe good ſtones may vary from the yellow colour of honey to a blackiſh brown. In this reſpect it is to be noted that the value of a ſtone does not depend on its colour, but on the uniformity of the tint, which becomes leſs intenſe when the ſtone is reduced into thin ſplinters. The flints of the two departments firſt mentioned are yellowiſh. Thoſe of the chalk hills on the banks of the Seine are blackiſh brown. Both the one and the other, when pulveriſed, are perfectly white. The ſilex pyromachus ought to poſſeſs an uniform ſemi-tranſparence, of a greaſy aſpect, to ſuch a degree, as to admit letters to be diſtinguiſhed through a piece of the ſtone of one-fiftieth of an inch thick, laid cloſe upon the paper. Its fracture muſt be ſmooth and equal throughout, and very ſlightly conchoidal; that is to ſay, convex or concave. This kind of fracture is one of the moſt eſſential properties upon which the faculty of being divided into gun-flints depends.

The

The workmen select the stones proper for their use by their external character. They compare that part of the masses of silex, which is uniformly semi-transparent and coloured, to the inner skin of bacon, which they call *couenne*. They say that one flint has more or less *couenne*, or is more or less fat than another. They assert that the upper part of a flint is always of a better quality than the lower. Flints are considered as imperfect or intractable when naturally deprived of any of the external characters before indicated, or when injured by long exposure to the air. Most of the masses are subject to have whitish opaque spots and a kind of knots where the material is harder than in the other part of the stone. When these accidents are too abundant, the stone is rejected as useless.

Selection of the best flints from their greasy aspect.

The physical characters are as follow:—Specific gravity of the white silex pyromachus from the banks of the Cher proved to be 26041; that of the blackish kind from the chalk isles of Laroche Guion was 25954. In this respect it does not differ essentially from the other varieties of silex, of which the specific gravities are usually between 26100 and 25900. Hardness a little superior to that of jasper, but inferior to that of agate and chalcedony. It is nearly the same as that of the other common flints. Brittleness. It is more brittle than most other siliceous stones. The light coloured is more so than the darker; these last being rather more scintillant, and and wear away the hammer more quickly. When two pieces of the silex pyromachus are strongly rubbed together, they emit the peculiar well known smell of siliceous stones, but in this variety it is more strong than in any other.

Physical characters. Specific gravity, hardness, brittleness, &c.

Chemical characters. Action of the air.

The silex pyromachus, deprived of its natural coating, and exposed for a long time to the changes of the atmosphere, acquires a second white friable coating, consisting of the silex reduced to powder; and even its internal part loses its greasy appearance and semi-transparency, so as to become whitish. In this case the specific gravity, which was 25954, becomes 25754, consequently it has lost 200 parts of the weight it possessed at first.

Chemical characters. Action of air.

The silex pyromachus is sometimes too moist when taken out of the quarry. It requires to be dried; but if by too long exposure to the air or the wind it loses a certain portion of humidity

It contains moisture.

humidity which is often very perceptible when it is recent, it can then no longer be broken into gun-flints, as its fracture is less easy. The workmen carefully reject all those which have lost this favorable degree of moisture. Perhaps they might be restored by keeping them in a damp place, or covering them with earth, and by these means at least they might succeed in preserving those intended to be worked up in winter.

Effect of heat. Oxidation.

When the fragments are thrown upon a red hot plate of iron it flies and cracks, and becomes opaque. When projected in powder upon nitre in fusion, it gives a few sparks with slight inflammation and detonation.

When calcined in a test it loses one 250th part of its weight, increases in bulk, becomes extremely white, and so brittle as to be almost friable. In this state it resembles the finest porcelain biscuit.

Distillation.

When distilled in a retort by strong heat it affords a little carbonic acid gas, and a quantity of water amounting to 200 parts of the weight before indicated as its specific gravity; but gives no sign of the combustible matter which in the preceding experiment caused the nitre to detonate.

The flint is an imperfect hydrophanes.

This water, which appears essential to all the flints, and may be called their radical water, is the cause of their transparency. Exposure to air by drying them renders them opaque; so that they may be considered as imperfect hydrophanes; for they do not again absorb, but with difficulty the water necessary to their transparence. This water also contributes to the connection of their integrant particles, whence their fracture becomes more equal, and is harsh when they have lost it. These flints when recently dug up even afford an aqueous vapour when struck, and the face of the fracture is humid, and as it were moist.

Chemical analysis.

Chemical analysis; fusion with potash; precipitation of the silex, &c.

Citizen Vauquelin examined 100 parts of silex pyromachus of a brownish colour, and uniformly semi-transparent from the hills of La Roche Guion. He mixed the mass with 400 grains of very pure potash, which by fusion in a silver crucible afforded a compound, which after cooling was diffused in water, and then super-saturated with muriatic acid. The very clear solution was evaporated to dryness, re-dissolved in water, and the silex thus saturated, and left upon the filter after being well washed,

washed, dried and ignited, weighed 97 grains. Ammonia was afterwards added to the clear liquid, where it produced a slight yellowish white precipitate, which after being well washed and dried weighed one grain, and was found to be a mixture of alumine and oxide of iron. The fluid separated from this small portion of iron and alumine, gave no other precipitate on the addition of carbonate of potash, and the waters used in the washing left no residue when they were evaporated to dryness. The result therefore was silex = 97 grains, alumine and oxide of iron = 1, loss = 2. The author considers it to be a very remarkable fact, that the silex pyromachus should contain only silex and water, the alumine and iron being too small in quantity to be considered as essential to its composition, or to influence its habitudes. Quartz also, from the analyses which have been made, appears to contain only silex; yet the more he examines the two substances, the more he finds reason to suppose them essentially different from each other; since the one refuses to assume the crystalline form, and the other assumes it and becomes clear, even in contact with the flint itself. The one appears to be incapable of admitting water into its composition, while the other constantly contains it until it begins to be decomposed. He offers a query, whether the difference may not consist in the small portion of combustible or fatty matter, which caused the detonation with nitre; or whether the quartz may not, like alum, acquire its property of crystallizing from the addition of some other substance. This question, as he remarks, must be resolved by future experimental enquiries.

Difference between silex and quartz.

The analysis of the whitish spots afforded silex 98 grains, oxide of iron 1, carbonate of lime 2. That of the absolutely opaque parts gave five parts more of carbonate of lime; and lastly, the analysis of the white coating naturally enveloping the masses, afforded 86 parts of silex, 1 oxide of iron, 10 carbonate of lime, and 3 loss. Those analyses which afforded no alumine, shew that this earth is not essential to the silex, and the absence of lime in the first analysis shews that it was an accidental ingredient in the latter.

Mineralogical situation. In France in the environs of St. Aignan, situated in the department of Loir-Cher, and in that of L'Indre, and the departments which occupy the vallies of Siene and Marne, are principally the places where this stone

Mineralogical situation.

ſtone is found. It exiſts in the chalky calcareous ſtones, in chalks more or leſs fine and ſolid, and in marles. They form horizontal ſtrata, by the manner in which the large and ſmall maſſes are placed beſide each other. Nevertheleſs, as the blocks of ſilex do not accurately touch each other, there is no ſolution of continuity between the upper and lower maſſes of chalk.

Out of twenty beds of ſilex lying one above the other, at a diſtance of twenty feet or leſs, there will frequently be no more than one, and very ſeldom two, which afford good ſtones of this deſcription; but in the bed which affords them, almoſt all the blocks have a greaſy appearance, and in the other ſtrata ſcarcely any of this deſcription will be found. Accordingly the good ſtrata are followed by ſubterraneous excavations, frequently at conſiderable expence, while the others are neglected.

On the banks of the Cher the flints are explored in a plain, by digging ſhafts to the depth of 40 or 50 feet, from whence horizontal galleries are carried into the only good ſtratum which is known.

On the banks of the Seine in the hills of La Roche Guion, the cliffs of chalk preſent ſteep precipices, where the ſtrata of ſilex are expoſed; and one of theſe ſtrata, which contains good ſtones for gun-flints, is about ſix toiſes from the upper ſurface of the great maſs of chalk.

Inſtruments.

Deſcription of tools or inſtruments.

The inſtruments uſed for faſhioning the gun-flints are four in number:

The mace.

1. A ſmall piece of iron or mace, with a ſquare head, Plate V. Fig. 1. the weight of which does not exceed two pounds, or perhaps a pound and a half, with a handle ſeven or eight inches long. This inſtrument is not made of ſteel, becauſe if it were too hard, its ſtroke might ſhatter the flint, inſtead of breaking it by a clear fracture.

The hammer.

2. A hammer with two points, in which the poſition of the points is of conſequence as to the nature of the ſtroke, Fig. 2. This hammer, which muſt be of good ſteel well hardened, and does not weigh more than ſixteen ounces; ſome do not exceed ten. It is fixed on a handle ſeven inches long, which paſſes through it in ſuch a manner, that the points of the hammer are nearer the hand of the workman, than the center of gravity.

gravity of the maſs. The form and ſize of the hammers of different workmen vary a little, but this diſpoſition of the points is common to them all, and is of conſequence to the force and certainty of the blow.

3. A little inſtrumen tnamed Roulette (roller) which repreſents a ſolid wheel, or ſegment of a cylinder, two inches and one third in diameter. Its weight does not exceed twelve ounces, it is made of ſteel not hardened, and is fixed on a ſmall handle ſix inches long, which paſſes through a ſquare hole in its center. Roulette.

4. A chiſſel bevelled on both ſides, ſeven or eight inches long, and two inches wide, of ſteel not hardened; it is ſet on the block of wood which ſerves as a work bench, out of which it riſes to the height of four or five inches. To theſe four inſtruments we may add a file, for the purpoſe of reſtoring the edge of the chiſſel from time to time. Chiſſel.

The proceſs:

After ſelecting a good maſs of ſilex, the whole operation may be divided into four manipulations. Manipulations.

1. To break the block. The workman being ſeated on the ground, places the flint on his left thigh, and ſtrikes it gently with the larger hammer, Fig. 1. to divide it into portions according to its ſize, that is to ſay, of about a pound and a half each, with broad ſurfaces nearly flat. He is careful not to crack or produce ſhakes in the flint by ſtriking it too hard. Break the block.

2. To cleave the flint, or break it into ſcales. The principal operation of this art is to cleave the flint well:—that is to ſay, to ſeparate from it pieces of the length, thickneſs, and figure, adapted to be afterwards faſhioned into gun-flints; and in this part the greateſt degree of addreſs, and certainty of manipulation are required. The ſtone has no particular direction in which it can be moſt eaſily broken. The courſe of its fracture depends entirely upon the choice of the workman. In this proceſs he holds a piece of flint in his left hand, not ſupported, and ſtrikes with the hammer, Fig. 2. on the broad faces produced by the firſt fracture, in ſuch a manner as to chip off the white coating of the ſtone in ſmall ſcales, and to lay bare the ſilex in the manner repreſented, Fig. 5.; after which he continues to ſtrike off other ſimilar portions of the pure ſilex. Theſe pieces are nearly an inch and a half wide, two inches and a half long, and one ſixth of an inch thick in the middle. Cleave or ſcale the flint. No particular direction of fracture.

They

Figure of the fcales.

They are flightly convex within, and confequently leave a fpace fomewhat concave, terminating longitudinally in two lines, fomewhat projecting, and nearly ftrait. The prominent edges produced by the fracture of the firft fcales, muft afterwards conftitute nearly the middle of the fubfequent pieces; and thofe pieces only, in which they are found, can be ufed to form gun-flints.

In this manner the operator continues to cleave, or fcale the ftone in different directions, until the natural defects of the mafs render it impoffible to make the fractures required, or until the piece is reduced too much to receive the fmall blows which feparate the pieces.

3. To fafhion the flint.

Fafhion or form the flint.

The gun-flint, Fig. 7. may be diftinguifhed into five parts, namely, 1. the edge, or bevel part, which ftrikes the hammer or fteel. This is two or three twelfths of an inch in width. If it were broader it would be too liable to break, and if more obtufe it would not afford a brifk fire. 2dly. The fide edges, which are always fomewhat irregular. 3dly. The back edge, moft remote from the hammer where the ftone poffeffes its intire thicknefs. 4thly. The under furface, which is fmooth and flightly convex. And 5thly. The upper face, which is flightly concave, and receives the action of the upper claw of the cock, in which it is fixed for fervice.

Operation.

In order to fafhion the ftone, thofe fcales or chips are felected, which have at leaft one longitudinal prominent angle. One of the two edges is fixed on to form the ftriking edge; after which the two fides of the ftone which are to form the fide edges, and that which is to form the hinder edge are fucceffively placed with the convex furface upon the edge of the chiffel, which is fupported with the fore-finger of the left hand, at the fame time that a fmall blow or two is given above the point of fupport with the Roulette, Fig. 3. by which the ftone breaks exactly along the edge of the chiffel, as if it had been cut. In this manner the fides and pofterior edge of the ftone are made.

Complete the edge

4. The ftone being thus reduced to its proper figure, the finifhing operation confifts in completing its edge in a ftrait line. For this purpofe the ftone is turned, and the under flat part of the edge is placed on the chiffel, in which fituation it is completed by five or fix fmall ftrokes with the Roulette.

The

The whole operation of fashioning a gun-flint is performed in less than one minute.

The rate of working. One thousand flints in three days.

A good workman can prepare a thousand good chips or scales in a day, if his flints be of good quality, and he can also fashion five hundred gun-flints in a day; consequently in three days he will cleave and finish a thousand gun-flints without further assistance.

Much refuse or waste.

This manufacture leaves a great quantity of refuse; that is to say, about three fourths of the whole stone: For there are not more than half the scales which prove to be well figured, and nearly half the mass in the best flints is incapable of being chipped out: so that it seldom happens that the largest piece will afford more than fifty gun-flints. The larger pieces of refuse are sold for the culinary purpose of striking a light.

The gun-flints when completed are sorted out, and sold at different prices, according to their degrees of perfection, from 4 to 6 decimes (or pence) the hundred. They are classed into fine flints and common flints; and according to their application into flints for pistols, fowling pieces, and muskets.

Local situation of the manufactories in France.

The fabrication and commerce of gun-flints in France is in some measure confined to three communes of the department of Loir-et-Cher, and one department of the Indre, as was before mentioned, namely, the commune of Noyers, 2,400 metres east north-east of St. Aignan; the commune of Couffy at 5,600 metres, and that of Meunes at one miriameter east south-east, and in the latter department, the commune of Lye 9 kilometers to the south-west of St. Aignan. The inhabitants of these communes employed in this species of industry are about 800, and they have excavated great part of the plain they inhabit.

Elsewhere practised.

A single workman named Stephen Buffet, who emigrated from the commune of Meunes to the banks of the Seine, where he has carried on this art for about thirty years by himself, was the person from whom Dolomieu obtained the present instructions. There are a few other places in France where this art is practised, but in none to the extent of the places before mentioned. The author has not met with this manufactory in any other countries, except in the territory of Vicenza, and one of the cantons of Sicily. He remarks that it may be carried on elsewhere, though probably overlooked by travellers, on account of its apparent insignificance. I believe it is practised at Purfleet, in the county of Kent, and in various other parts of England.

III.

Letter from Dr. BEDDOES *on Prognoſtics of the Weather, the Effects of the Nitrous Oxide, and other Objects.*

To Mr. NICHOLSON.

SIR,

Clifton, Jan. 9, 1802.

Probability that the enſuing ſpring and ſummer may prove cold.

SINCE the late fall of ſnow, I have been aſked by ſeveral people, how I would now apply the principle, ſtated in a letter, inſerted in one of your former Numbers *. My anſwer was, that it induced me to ſuſpect a correſponding great fall in the N. and N. E. regions. If this be the fact, and if no unknown calorific proceſs take place between us and the equator, it would furniſh an unfavourable prognoſtic with regard to the weather of the enſuing ſpring and ſummer.

How the weather is to be foreknown.

We can, I ſuſpect, only attain a degree of preſcience concerning the ſeaſons, by comparing the order of the moſt obvious meteorological events. This, you know, was the way in which the firſt improvements were made in aſtronomy. In order to gain another point of compariſon, I beg leave diffidently to propoſe a conjecture, reſpecting the duration of different falls of ſnow. The duration, I ſuppoſe, will bear ſome proportion to the quantity on the ground. For example, *if an inch of ſnow fall, when the thermometer has been within the twelve preceding hours above 32°, it will either lie a much ſhorter time than if ſix inches fall, ſo as to lie under the ſame condition.* Perhaps the duration will generally be at leaſt equal to the ſquares of the depths.

Conjecture.

Atmoſpheric currents of different temperature.

You will ſuppoſe that I found my conjecture upon ſome ſort of reaſoning. I deduced falls both of ſnow and rain, after Dr. Hutton, from the mixture of airs, unequally heated. I conſider ſnow falling and lying in quantity, as the ſign of a great current of air from the polar regions, particularly when the wind has blown for ſome time from the oppoſite points;

* Philoſ. Journal, Quarto V. 131. The principle is that, *If there be an unuſual fall of ſnow in the countries on the N. and N. E. our ſummer, cæteris paribus, will be cold and wet.*

that

that is, when a large body of air, at above 32°, has gone to the northward of these islands. If the snow lie to any depth, I imagine this to be a sign of the preponderance of an under-current from the same quarter, which will generally produce a frost of some continuance, as well as snow in the first instance. Where a S. or S. W. wind prevails, the snow will change into rain, and not lie to any depth.

I abridge this precarious speculation, (which it would be easy to extend into a pamphlet) and suppress many explanations, in order merely to propose, as a query, *Whether a snow, like that which now covers the earth, be not always actually the forerunner of a frost of some days, and generally of some weeks, continuance?* Query.

During the present week we had the curious phenomenon of a thaw for (I believe) near 48 hours, with the wind at N. E. which no doubt arose from a considerable upper current from the contrary quarter, at no great elevation. But the quantity of snow, already on the ground, made me venture repeatedly to say, that the weather would not soon change to a compleat thaw. On the intermediate thaw.

In a short time I intend to put to press a report of the operations at the pneumatic institution here. The nitrous oxide has justified the expectation, originally formed of its powers in palsy, and also as a *restorative*, if I may employ this abused word, to certain constitutions uniformly broken down, and has proved an admirable *soother* of the sufferings of old age. We (I speak of myself and other persons, concerned at the institution) have frequently witnessed in new patients the temporary restitution of the voluntary power over a perfectly paralytic limb during the act of inhalation. This power has become permanent afterwards in some instances, but not always, though perhaps not from want of efficacy in the air. It has also been discovered here, that oxigen gas, long inhaled, affects the growth and the internal parts, especially the bones, in a very extraordinary manner. The effect on the bones I had anticipated, and the altered growth, seems a natural consequence. Some accurate drawings by Mr. King will serve to exhibit these effects. Medical effects of the nitrous oxide, &c.

We ſhall have to report alſo ſome conſiderable improvements in medicine from the uſe of new, or little known remedies. Will you allow me to mention, that I ſhould be glad to engage with an active and ardent young enquirer, who has already ſome readineſs in common chemical proceſſes. His employment would be in reſearches, connected with medicine and phyſiology. If ſuch a perſon, reſident in London, ſhould be induced by this notice to inquire, he might obtain ſome previous ſatisfactory information from Mr. Davy at the Royal Inſtitution.

Wiſhing your improved Journal the ſupport it merits,

I am, SIR,

Reſpectfully your's,

THOMAS BEDDOES.

IV.

On the Quantity of Nutriment to be obtained from various Kinds of Bones, and the beſt Method of extracting it, by Profeſſor PROUST.

Prouſt on improving the ſubſiſtence of the ſoldier.

MR. PROUST, profeſſor of chemiſtry at Madrid, has publiſhed a work, intitled an Inquiry into the Means of Improving the Subſiſtence of the Soldier. The following is a ſummary of what it contains of moſt importance, and of general utility.

Jelly from bones.

It has long been known, that a very agreeable and nutritious food might be procured from bones: a food particularly recommended as a reſtorative under the name of hartſhorn jelly, which is in fact procured by boiling ſhavings of bones, the common hartſhorn ſhavings of the ſhops. All that is wanting, is a cheap and eaſy mode of procuring this jelly in ſufficient quantity from a ſubſtance now commonly thrown away; for bones after they come out of the pot are as good for the purpoſe, and nearly as productive, as freſh bones. The ſhavings are too dear. Papin's digeſter, which has been employed, is a machine too expenſive for the poor, too dangerous for common uſe, and apt to give the jelly a burned taſte.

To

Mode of procuring it.

To obviate all theſe objections, nothing more is neceſſary, than to reduce the bones to powder; which may be very readily done between a pair of toothed iron cylinders, as in the ammoniac works. The bones thus comminuted are to be boiled in eight or ten times their weight of water for the ſpace of four hours, or till about half the water is waſted, when the liquor will be found on cooling of a due gelatinous conſiſtence. A veſſel with a tight cover ſhould be uſed, that the water may acquire as much heat as poſſible; and it ſhould not be of copper, as this metal is eaſily diſſolved by animal mucilage.

Proportions of jelly in different bones.

It is to be obſerved, that bones from different parts afford different proportions of jelly, by which of courſe the quantity of water ſhould be regulated. According to the experiments of Mr. P. five pounds of the middle part of the bone of a leg of beef will afford nine pints of jelly; the ſame quantity of the bone of the joint, fifteen pints; of the ribs and ſpine, eleven quarts; of the rump and edge bone, thirteen quarts. Five pounds of mutton bones of every ſort together give nineteen pints of jelly. Pig's bones yield a little more. To Mr. P's taſte, the jelly from pig's bones was the moſt agreeable of all: that from mutton had the diſtinguiſhing flavour of the meat. Of the jellies from beef bones that from the ribs was moſt pleaſing, both to the ſight and palate, that from the leg and joints leaſt.

In warm weather the liquor muſt be boiled down ſomewhat more, if it be intended to aſſume the ſame gelatinous conſiſtence when cold; as the ſame quantity of bone, that would afford a quart of jelly in winter, will not yield above a pint and a half, or a pint and a quarter in ſummer, but then it contains proportionally more nouriſhment.

Portable jelly

If this jelly be boiled till it acquires a conſiſtence a little thicker than a ſyrup, then poured out into plates, and when cold cut into pieces and dried on a net, it will keep a long time, and be particularly uſeful at ſea. One ounce of this dry portable jelly, being ſoaked in water for a quarter of an hour to ſoften it, and then boiled, will make from a pint and a quarter to a quart of jelly, according to the ſeaſon, and equally as good as that which is freſh extracted.

Mr. P. prepares a very pleaſant reſtorative for the ſick, as he informs us, by adding an ounce and a half of ſugar, and a little ſalt, to fourteen or fifteen ounces of the jelly, and then making

making it into an emulsion with twelve sweet and four bitter almonds, and a little orange peel.

Suet obtained from bones.

Another advantage to be derived from these refuse bones is the fat. Before grinding them to extract the jelly, Mr. P. chops them into pieces about an inch long with a cleaver, throws them into a kettle of boiling water, and lets them boil about a quarter of an hour. The fat obtained in this manner from sixteen pounds of rump and edge bones weighed, when cold, two pounds; and from the same quantity of the bones of the joints he obtained four pounds of solid fat. This, he observes, when fresh, may be used for various culinary purposes; and when it has been kept for some time exposed to the air, it becomes very good tallow for making candles.

V.

Observations on the Effects which take Place from the Destruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafness. By Mr. ASTLEY COOPER.*

Injury or loss of the membrana tympani does not occasion deafness.

AFTER referring to a former paper, in which he had proved, by facts, that an aperture in the membrana tympani does not diminish the faculty of hearing, and that a complete destruction of the membrane does not occasion a total loss of that sense, Mr. Cooper observes, that there are various causes by which the membrane may be injured or destroyed. Of these the most common is suppuration in the meatus auditorius. In persons of a delicate constitution and irritable habit the ear-wax is liable to be hardened, gradually bringing on deafness, and then exciting inflammation and suppuration. If this be not remedied, not only the membrane lining the meatus, but that of the tympanum also, will be destroyed, the small bones of the tympanum be discharged, and sometimes considerable exfoliations take place. At Fig. 4, Pl. VI. is a view of the membrane of the tympanum lacerated by a blow on the head; probably from the air being driven into the meatus with violence. Children often introduce small stones, pieces of slate pencil, and even pins, into their ears,

Various causes of its loss or injury.

* Abridged from the Philosophical Transactions, 1801, p. 435.

in

in extracting which confiderable lacerations have been made in the membrana tympani. Fig. 5 fhews one torn in attempting to remove a pin.

The membrana tympani may be eafily feen in fome perfons, by directing the rays of the fun, or a condenfed light from a common lamp, into the ear: bnt this is not the cafe in all; for the meatus differs confiderably in different perfons, both in its depth and diameter. If the ear be clear of wax, the membrane has a bright tendinous appearance; and an aperture in it appears as a dark fpot, which, by the filvery furface of the membrane furrounding it, is rendered diftinctly perceptible. If there be an aperture, air alfo, upon blowing the nofe with violence, will be forced with a whiftling noife through the ear; the fmoke of tobacco may be driven from the mouth through the ear; or water may be injected from the ear into the throat.

How the membrane may be feen, and its appearance. Means of difcovering an aperture in it.

If the whole of the membrane be deftroyed, and three out of four of the fmall bones of the tympanum be removed, an almoft total deafnefs enfues; but the ear, after a time, begins to recover its powers, and, in the end, regains them, with that degree of imperfection only, which was defcribed in the cafe of Mr. P——, in Mr. Cooper's former paper. When the membrane of one ear only is deftroyed, a greater degree of deafnefs takes place in that ear, than would happen in either, were the membrane deftroyed in both. This probably arifes from the difufe, into which the imperfect ear falls. Thus Mr. G——, at an early period of life, loft a great portion of the membrana tympani of the left ear; and as he heard fomewhat better with his right ear, he was little in the habit of employing the left, till at length he confidered himfelf almoft totally deaf in it. Becoming deaf in the right ear, however, and confequently obliged to employ the left, he found it by no means deprived of its powers.

Effects of the deftruction of the membrane.

Fig. 3.

Thefe obfervations have induced Mr. Cooper to try the effect of an operation, which has proved fuccefsful in feveral inftances of one fpecies of deafnefs,—that which arifes from obftruction of the Euftachian tube. Of this obftruction there are feveral caufes. A common cold, affecting the parts contiguous to the orifice of the tube, may prevent the free paffage of the air into the tympanum. The deafnefs thus occafioned is in general temporary; but it may become permanent,

Cure of a fpecies of deafnefs.

The Euftachian tube may be obftructed from feveral caufes.

manent, if a frequent recurrence of ſuch attacks produce permanent enlargement of the tonſils. The ſcarlet fever occaſions ulcers in the throat, which, in healing, frequently cloſe the Euſtachian tubes. A venereal ulcer may have the ſame effect. The tube has been cloſed by an extravaſation of blood in the tympanum. And in one inſtance a ſtricture in the tube rendered the paſſage extremely difficult. In this caſe, by forcing air from the mouth into the cavity of the tympanum, and then preſſing gently on the ear to expel part of the air from the cavity, an immediate increaſe of the power of hearing was produced. Theſe are the moſt common cauſes of obſtruction in the Euſtachian tube; and Mr. Cooper has reaſon to think, from the experience he has already had, that they may all be remedied by puncturing the membrana tympani. In ſupport of this opinion he gives four ſelect caſes, one of which is the following:

Caſe of deafneſs cured by puncture of the membrane of the tympanum.

Mr. Round, of Colcheſter, conſulted Dr. Baillie reſpecting his ſon, Mr. John Round, aged ſeventeen, who had laboured, from his birth, under ſuch a degree of deafneſs, as would have incapacitated him from engaging in buſineſs. Dr. Baillie, having ſatisfied himſelf that there was no nervous defect in the ear, referred him to Mr. Cooper. He found that this gentleman had been born with an imperfect ſtate of the fauces, which rendered him incapable of blowing his noſe; that the Euſtachian tubes had no openings into the throat, and, therefore, that he was unable to force air from the mouth into the ear. The auditory nerves, however, were perfect; for he could diſtinctly hear the beating of a watch, if placed between the teeth, or againſt the ſide of the head; and he had never perceived any buzzing noiſe in his ears. Our author therefore adviſed him to ſubmit to the operation of perforating the membrana tympani; to which he cheerfully conſented. The moment this was done, a new world was opened to him; and the confuſion produced by the number of ſounds, which immediately ſtruck his ear, made him ſink upon a chair, almoſt in a fainting ſtate. From this ſtate he recovered in about two minutes; and, finding that his hearing was completely reſtored upon the one ſide, he wiſhed the operation to be performed upon the other; which was immediately done, with the ſame happy reſult, and without his experiencing the ſame confuſed ſenſation as before. Near two months after the

operation,

operation, Mr. Cooper had the pleaſure to receive an aſſurance from him, that he had ſuffered no relapſe, nor any inconvenience from the openings which had been made, and that his hearing continued perfect.

The operation conſiſts in paſſing into the ear a canula, of the ſize of a common probe, in which a trocar is concealed: the canula is to reſt upon the membrana tympani, and the trocar is then to be thruſt through the membrane. The trocar ſhould be ſo adjuſted, as not to paſs more than one-eighth of an inch beyond the canula, to prevent its reaching the oppoſite ſide of the cavity of the tympanum. Should it however touch the perioſteum of the tympanum, it can be productive of no ſerious harm. The aperture ſhould be made in the anterior and inferior part of the membrane, under the handle of the malleus, which muſt not be injured in the operation; and it is therefore neceſſary, that the operator be acquainted with the exact ſituation of this bone. Though the membrana tympani is vaſcular, the veſſels are ſo ſmall, that they bleed but little; and therefore, if much blood be diſcharged, the operation cannot have been properly performed. In an ear otherwiſe healthy, the operation is attended with ſo ſlight a degree of pain, that, when it has been performed in one ear, the patient expreſſes no unwillingneſs to ſubmit in the other. The ſenſation it occaſions is momentary; and no ſubſequent inconvenience of any kind ariſes: though if the ear have been previouſly irritated by ſtimulating applications, the operation will be painful, and ſhould be deferred, till the inflammation has ſubſided. **Mode of performing the operation.**

As this operation affords relief only in caſes of cloſure of the Euſtachian tube, attention ſhould be paid to the following criteria of this defect. **Caſes in which it affords relief.**

If a perſon, on blowing the noſe violently, feel a ſwelling in the ear, from the membrana tympani being forced outward, the Euſtachian tube is open. Though the tube be cloſed, if the beating of a watch placed between the teeth, or preſſed againſt the ſide of the head, cannot be heard, the operation cannot relieve, as the power of the auditory nerve muſt have been deſtroyed. In a cloſed Euſtachian tube there is no noiſe in the head, like that accompanying nervous deafneſs. Laſtly, it is right to inquire, whether the deafneſs were immediately preceded by any complaint in the throat. **Criteria for diſtinguiſhing them.**

The

The caufes of deafnefs are numerous, and many cafes may be relieved by furgery.

The caufes of deafnefs are extremely numerous; and many of thofe which affect only the meatus auditorius, the membrana tympani, the cavity of the tympanum, and the Euftachian tube, admit of relief from furgery.

Nervous deafnefs.

But there is one fpecies, in which it would be abfurd to expect this from any operation upon the membrana tympani. This occurs more frequently than any other, happening generally in old perfons; but fometimes, in the delicate and irritable, in the earlier ftages of life. Anxiety and diftrefs of mind have been known to produce it. Its approach is generally gradual: the perfon hears better at one time than at another; a cloudy day, a warm room, agitated fpirits, or the operation of fear, will produce a confiderable diminution in the powers of the organ. In the open air the hearing is better than in a confined fituation; in a noify, than in a quiet fociety; in a coach when it is in motion, than when it is ftill. A pulfation is often felt in the ear; a noife, refembling fometimes the roaring of the fea, and at others the ringing of diftant bells, is heard. This deafnefs generally begins with a diminifhed fecretion of the wax of the ear, which the patient attributes to fome unufual expofure of the head to cold; and

May be cured in the commencement by ftimulants.

this continues as long as the difeafe remains. It may be cured in the commencement by the application of fuch ftimulants, as are capable of exciting a difcharge from the ceruminous glands; for which purpofe they fhould be introduced into the meatus. But if thefe be ufed fo as to irritate, without exciting a difcharge, they are rather prejudicial than otherwife. In cafes of this kind the operation has afforded no farther relief than diminifhing the noife in the head.

Deafnefs from the generation of a folid fubftance in the labyrinth.

Another caufe of deafnefs is probably irremediable. This is the generation of a folid fubftance in the labyrinth of the ear, inftead of the water with which it is naturally filled. Mr. Cline, diffecting the head of a young man born deaf, and confequently dumb, found all the parts of the organs of hearing perfectly formed, but the veftibule, cochlea, and femicircular canals, were filled with a fubftance of the confiftence of cheefe.

Thefe inftances of deafnefs Mr. Cooper thought it right to defcribe, becaufe they are liable to be confounded with that which arifes from a clofed Euftachian tube.

EXPLANATION OF THE FIGURES. Plate VI.

Fig. 1 ſhews the external ear, the meatus auditorius, membrana tympani, and Euſtachian tube.

A, the meatus.

B, the membrana tympani.

C, the cavity of the tympanum.

D, the Euſtachian tube.

Fig. 2 ſhews the perforating inſtrument as it is introduced in the operation.

Fig. 3 the membrana tympani of Mr. G——, of which only that part which appears of a lighter colour remains.

Fig. 4 the membrane lacerated by a blow.

Fig. 5 the membrane lacerated in an attempt to extract a pin.

Fig. 6 ſhews the membrana tympani of a medical man in the city, having a fungus projecting through it. In this ear he is conſiderably deaf.

Fig. 7 the other membrane of the ſame gentleman.

Fig. 8 one of the membranes of Mr. P——, whoſe caſe was deſcribed in the former paper.

Fig. 9. A membrana tympani in its natural ſtate, ſhewing the attachment of the manubrium to the malleus.

Fig. 10 the appearance of the membrane after having been punctured.

VI.

Note reſpecting the Abſorption of Nitrous Gas, by Solutions of Green Sulphate and Muriate of Iron. By Mr. H. DAVY, *Director of the Laboratory, and Lecturer on Chemiſtry to the Royal Inſtitution.—(From the Author.)*

WHEN nitrous gas is brought in contact with ſolution of green ſulphate, or green muriate, of iron, it is rapidly abſorbed; the colour of the fluid alters; and it becomes, when ſaturated with the gas, dark brown, and almoſt opaque.

Solution of green ſulphate, or muriate of iron, abſorbs nitrous gas;

Solution of ſulphate, or muriate, of iron, impregnated with nitrous gas, apparently undergoes no change at low temperatures, when preſerved from the contact of the atmoſphere. But

and undergoes no change at low temperatures.

unlefs expofed to the air, when it abforbs oxigen, becomes acid, depofits red oxide, and affords a little ammonia.

But if it be expofed to it, it rapidly abforbs oxigen, lofes its colour, and becomes acid to the tafte. In this cafe, the green oxide of iron is converted into red oxide; and a fmall quantity of ammonia is formed.

The impregnated folution, *in vacuo*, emits its nitrous gas without change.

When a folution of fulphate of iron, impregnated with nitrous gas, is placed under the receiver of an air pump, in proportion as the preffure of the atmofphere is removed from it, it gives out its nitrous gas. When the mercury in the gage ftands at about $\frac{3}{10}$ of an inch, it becomes almoft wholly freed from it, recovers its colour, and is found unaltered in its properties.

The fame folution, heated, emits nitrous gas, depofits a little red oxide, and contains ammonia.

When folutions of green fulphate, or muriate, of iron, impregnated with nitrous gas, are expofed to the heat of a fpirit lamp, nitrous gas is produced from them in a very pure ftate, the fluids lofe their dark colour, and a fmall quantity of yellow oxide of iron is thrown down in them. If examined after this procefs, they are found to contain a little ammoniac, combined with fome of the acid; and the oxide of iron diffolved is green oxide.

Theory.

The theory of thefe appearances is obvious.

The nitrous gas is firft abforbed by fimple affinity; probably from the attraction of the iron for oxigen.

The abforption of nitrous gas, by folutions containing oxide of iron, at its minimum of oxidation, at common temperatures, appears to be owing to a fimple combination between the gas and the fluid; owing probably in fome meafure to the modified affinity of the green oxide of iron for oxigen; for the red fulphate and muriate of iron have no affinity for nitrous gas.

Expofure to air converts the nitrous gas into acid; a portion of which, and alfo of the water, are decompofed by the iron converted into red oxide.

The changes taking place in the impregnated folutions, when expofed to the atmofphere, evidently depend upon the converfion of the nitrous gas of the folution into nitrous acid, by the abforption of oxigen; and upon the fubfequent decompofition of a portion of this nitrous acid, and of the water of the folution, by the green oxide of iron.

Heat increafes the tendency of nitrous gas to fly off, and alfo that of the iron to decompofe it, and water. Hence red oxide is formed by the oxigen of this gas and of the

Heat feems to act upon impregnated folutions, by increafing the tendency of nitrous gas to affume the elaftic form, and by increafing the affinity of the green oxide of iron for oxigen. In confequence, a portion of nitrous gas flies off, and another portion is decompofed; its oxigen combining with the green oxide, and its nitrogen with the hidrogen of a fmall quantity of water, which is acted upon by a fimilar attraction.

The abforption of nitrous gas by folution of common ful phate of iron was difcovered by Dr. Prieftley. It has been employed

employed by Mr. Humboldt for the purpofe of afcertaining the quantity of nitrogen mingled with nitrous gas; and that experimentalift, in conjunction with Mr. Vauquelin, has attempted to prove that the procefs of abforption is owing to an immediate decompofition of the nitrous gas by the water of the folution; in confequence of which nitrate of ammonia is formed.

water, and ammonia by its nitrogen with the hidrogen.

Hiftory. Humbold and Vauquelin think the gas is completely decompofed at its firft abforption, and ammonia formed.

This conclufion is publifhed in the 28th volume of the *Annales de Chimie*. In the 39th volume of the fame work, 30 Meffidor, An 9. p. 1. Mr. Berthollet ftates, that nitrous acid is probably formed during the abforption of nitrous gas by folution of fulphate of iron; in confequence of the evolution of a portion of its nitrogen, and in confequence of the more intimate union of its remaining conftituent principles. Yet, in the fame paper, he details an experiment on the application of heat to an impregnated folution, which was attended with refults fimilar to thofe defcribed above; and in confequence of which he is induced to enquire, "If the fulphate of iron is capable of exerting two different actions in nitrous gas; one tending fimply to abforb it, and the other to decompofe it?" p. 11.

Berthollet apprehends a partial decompofition;

but neverthelefs is led to fufpect the differences of action from temperature.

The general account that has been given in this Note of the true nature of the procefs, is extracted from "Refearches, Chemical and Philofophical, concerning Nitrous Oxide, publifhed in June, 1800." The experiments upon the abforption were made under Mercury; and, in confequence, the agency of the atmofphere, which appears to have mifled the celebrated foreign chemifts juft quoted in their conclufions, was avoided.

The author's experiments were made over mercury; and confequently he was not, like thofe chemifts, mifled by the action of the atmofphere.

VII.

On certain Metallic Sulphurets. By PROUST, *Profeffor of Chemiftry at Madrid* *.

I LONG fuppofed, that the iron in pyrites was oxided at the minimum, and, fupporting my opinion by fome analogies, I publifhed it in the year 1795; but when my attention was recalled to the fubject, by reading the Memoir which Vauquelin

Ill founded opinion, that the iron in pyrites is oxided.

* Journal de Phyfique. Thermidor. 9.

quelin has published on the sulphurets, I thought I ought no longer to delay making known the facts which have induced me to adopt a different opinion. I made use of the fine cubical and dodecahedral pyrites, which are obtained in abundance from an indurated argill, found in the vicinity of San Pedro Marino, near the town of Soria.

Distillation of pyrites

Four hundred grains of entire pyrites of the size of hemp-seed, distilled by a red heat, yielded,

Of Pure sulphur	-	75 grains
Residuum	-	322
Loss	-	3
Total		400

Four hundred grains of the same of a little larger size, yielded,

Sulphur	-	81 grains
Residuum	-	314
Loss	-	5
		400

A moderately red heat is sufficient for extracting all the sulphur which they are able to yield. In the first moments, a small quantity of aqueous vapour passes over; afterwards, two gases, which may easily be recognised by their smell, namely, the sulphureous and hydrogen-sulphureous, which mutually decompose each other in the water of the receiver, and render it milky. Both are owing to the decomposition of the humidity; the sulphur passes over afterwards, and the gas is no longer perceived.

converts them into sulphuret.

The abstraction of about 20 parts in 100 of the sulphur from the pyrites, destroys in a great measure their metallic lustre and solidity; and though they preserve their form, they are more voluminous, melted on all sides, and may be crushed between the fingers; they have only the dull colour of an artificial sulphuret.

They contain no oxigen.

Hence it is evident, on comparing the products, that if oxigen constituted part of the pyrites, we must seek it in the residuum of their distillation; but the following facts annihilate the hope of finding it there.

Proof.

When we treat the different oxides of iron with sulphur, or cinnabar, in a red heat, the results are, sulphureous gas, and common sulphuret of iron. If therefore the pyrites contained oxigen,

oxigen, it is evident that the latter could not escape the attraction of the combustible substance in a high temperature.

When treated with carbon, pyrites produces only sulphurated hidrogen; but if first deprived of 12 or 20 hundredths of of sulphur, it presents only sulphuret, or iron saturated with sulphur, in the proportion of 60 to 100. It is soluble in the sulphuric, muriatic, and dilute nitric acids, affording sulphurated hidrogen in abundance.

Pyrites is a sulphuret overcharged with sulphur.

Pyrites, therefore, according to these results, is nothing more than a sulphuret formed by nature in the humid way, and surcharged with an excess of sulphur, as if to ensure the duration of her work.

They differ much; and only yield their surplus by distillation.

The pyrites will undoubtedly be found to differ greatly from each other with respect to this excess of sulphur; for, according to Henkel, there are some which yield 25,28, even 32 per cent. of sulphur. It is therefore to this surplus that the product of this distillation belongs, and not to the sulphur, which has been combined with iron by the invariable law of the proportions; for the metal, when saturated with $\frac{60}{100}$ of this combustible, yields none on being exposed to that temperature which deprives the pyrites of its excess.

Easy method of procuring hidrogen.

The pyrites, besides their different known uses, are of great use in a laboratory. We may fill a crucible with them, cover the powder with about a third part of filings, sprinkle over these a little charcoal powder, and afterwards bring the whole to a red heat, without carrying it so far as to reduce them to the state of fusion. In this manner we obtain a homogeneous mass, very convenient for procuring hidrogen in abundance.

The least sulphurated pyrites are soonest vitriolized.

By what I have just said, I do not however mean to imply that all the pyrites resemble each other. Those, for example, which possess the property of becoming vitriolized, approach perhaps the nearest to the sulphuret of iron without excess; for it is certain that the pyrites most charged with sulphur are at the same time those which resist the tendency of the elements to destroy them the longest.

The pyrites of Soria contains a little chalk, sand, and argill, but not the slightest trace of copper: its solution in the nitric acid is reduced to the minimum of oxidation by means of sulphurated hidrogen; and the yellow powder which separates from it is nothing but sulphur.

The

Natural production of red ochre.

The pyrites undergo a flow decompofition, by which oxigen is fubftituted in the place of the fulphur, without their lofing any thing of their dimenfions; they are then nothing more than red oxide. When the waters tranfport, divide, and attenuate the oxided pyrites, they become changed into red ochre: of this kind is that which in Spain is termed *Almagné*, from the name of the village where it is found. It is employed for painting houfes, marking fheep, colouring the Seville fnuff, polifhing glafs, &c. This earth, when diftilled, yields fulphureous acid in abundance: it announces its origin by the remnant of fulphur which it contains.

Yellow ochre.

As to the beautiful yellow ochres, which acquire a red colour by merely lofing their humidity, they proceed from the fpontaneous decompofition of the fpathofe ores: we therefore find them to contain lime and manganefe. I have obferved this converfion of the fpathofe ore into the ftate of yellow ochre in a very diftinct manner in certain veins; that is to fay, the oxides of iron and manganefe, when both at the minimum, and faturated with carbonic acid, raife themfelves to the maximum, by abandoning the acid, which can no longer remain united with them in this ftate.

Umber.

Analyfis of umber fhews it to be vegetable.

If this earth be a vegetable refidue, as cannot be doubted, its origin is well confirmed by its analyfis, for it is a compound of oxide of iron and manganefe, both at the maximum, of argill, of fand, &c. As to the argill, it abounds in the afhes of certain kinds of wood; I have found it in large quantity in that of the olive and careb trees; in that of the holm (*encina* in Spanifh) I have found the calcareous phofphate, which I neglected to announce at the proper time. In addition to my fecond memoir on Pruffian blue, I have to add, that ox's blood certainly contains manganefe.

Sulphuret of Mercury.

Mercury in cinnabar is not oxided.

Artificial cinnabar conftantly yields 85 per cent. of mercury on the one hand; on the other it relinquifhes 14 and $14\frac{1}{2}$ of fulphur to the antimony which is ufed for effecting its decompofition. The mercury is not therefore oxiginated in cinnabar.

In

In my youth I made two experiments, which might have coſt me very dear. I wiſhed to convert mercury into cinnabar in the dry way, as I had done by employing the oxides in the humid way. Four ounces of corroſive muriate, and four ounces of red oxide, triturated with ſulphur, were expoſed to the fire, each mixture in a ſeparate retort. In leſs than half an hour, two exploſions took place, which were heard throughout the extenſive works of La Salpêtrière. My brother, who conducted the firſt experiment, being ſuddenly overwhelmed with a thick ſmoke of ſublimate, had nearly loſt his life, and his lungs were long afterwards diſordered. The reſult of my operation was, that the dome of the furnace was thrown into the air, whilſt the door of the aſh-pit was ſhattered againſt a wall, and narrowly eſcaped ſtriking me in the ſtomach.

Dangerous exploſions, by attempts to form cinnabar with oxided mercury, by heat.

The powder which aroſe from the detonation of the oxide mixed with ſulphur, according to the method of Bayen, is cinnabar of a violet-red colour.

The æthiops, prepared with the aid of fire, reddens ſpontaneouſly in courſe of time, in the oxiding veſſels.

Mercury poured into a retort with melted ſulphur always occaſions an inflammation; but if both have been previouſly heated, the union takes place, and the æthiops affords cinnabar without flame. As to that which accompanies the proceſs uſed by the Dutch, it deſerves to be better examined than has hitherto been done.

On the inflammation of the common proceſs.

Hydro-ſulphurated water, poured gradually into a ſolution of ſublimate, decompoſes it entirely. The precipitate is mild muriate, and muriatic acid is diſengaged. This reſult proves, that there is not only more oxigen in the oxide of ſublimate, but alſo, as Berthollet has remarked, more marine acid. If, on the contrary, we pour the ſolution of ſublimate into the hydro-ſulphurated water, the whole becomes precipitated in the form of æthiops, and the muriatic acid remains alone.

Corroſive ſublimate contains more oxigen and more acid than the mild muriate.

Remarkable fact.

The name of *ſweet* muriate brings to my recollection a fact recorded by Lemery in the Memoirs of the Academy. He knew an alchemiſt, who eat the mild muriate like bread; he has ſeen him ſwallow four ounces at a time, and he aſſured him that he took from time to time a like doſe, to purify his blood, as he ſaid. This anecdote, the authenticity of which cannot be ſuſpected, induces me to aſk what we are to think of the 18 and 24 grains of *mercurius dulcis* which phyſicians are

Anecdote, to ſhew the inefficacy of mild muriate as a medicine. *It was not the muriate.* W. N.

are in the daily habit of prefcribing? I greatly fear that it may be much the fame with regard to its effects as with thofe of fedative falt, and fo many other medicines, which have nothing to recommend them, but the honour which is done to them, of affording them a place, from age to age, in our difpenfatories and catalogues of the *materia medica*.

Mild muriate treated with fulphur;

I find alfo in my notes of the fame period, that the mild muriate, treated with fulphur in a retort, affords cinnabar and corrofive muriate; the mild muriate is therefore divided into two portions; one of which is converted into cinnabar, and the other charges itfelf with the oxigen and acid of the firft.

alfo turpeth.

That turpeth mineral, treated in the fame manner, is changed into cinnabar, and quits the oxigen, which goes to form fulphureous acid.

The concentrated fulphuric acid decompofes cinnabar, like the pyrites and other fulphurets, yielding to the metal the oxigen which they require; hence the fulphureous acid, &c.

Cinnabar in the humid way.

Mercury prefers fulphur to oxigen.

Mercury having much more affinity for fulphur than for oxigen, never fails to fhew its preference for this combuftible. It is not fubject to the law which enables zinc, tin, antimony, &c. to combine with fulphur, without abandoning the oxigen.

Liquid fulphurets decompofed by crude mercury;

F. Hoffman was, I think, the firft who thought of decompofing the ammoniacal fulphuret by means of mercury. Baumé and Wiegleb have fince occupied themfelves with the fubject, and in a more extenfive manner.

which takes the fulphur, and becomes cinnabar.

Mercury, poured into flafks containing fulphuret of potafh, or of ammonia, decompofes them entirely, and reduces them to potafh and ammonia: in the fpace of a few days it paffes from the black to the red colour. If mercury required oxigen in order to affume the colour of cinnabar, whence fhould it take it? This procefs is certainly of a very difoxidating nature.

Mercurial falts and oxides alfo.

The nitrates, muriates, fulphates, and mercurial oxides of every kind, all yield æthiops in the firft moment of their mixture with the fulphurets: they all become violently heated. In order to accelerate their coloration, it will be fufficient to place the flafk with the mixture upon burning coals; in an inftant they begin to grow red at the points where the heat exerts its action. Is it fulphurated hidrogen which they then lofe? For my part, I cannot tell.

All

All theſe cinnabars differ with reſpect to their ſhades of colour and fineneſs; ſome are of a tinge approaching to purple, violet, &c. That of ſublimate combines with a great degree of tenuity, a vivid ſcarlet brilliancy. It is much ſuperior to the richeſt vermilions, and it is to be wiſhed that painters might become acquainted with it. These cinnabars differ. A very beautiful pigment.

Others are pulverulent, of a dark or dull colour; ſuch are thoſe which the ſulphuret of potaſh yields; they have neither the luſtre nor the tenuity of the others, for they are cryſtalline. For the reſt, all theſe cinnabars preſent no extraordinary appearances in ſublimation; by this proceſs they loſe the brilliancy which they owed to their ſtate of diviſion.

Tin, which has ſo great an affinity with oxigen, takes however nothing elſe than ſulphur from cinnabar; if the latter contained alſo oxigen, it would take it from it with the ſame facility as it does the ſulphur; and the preſence of the one would certainly be no impediment to the tin becoming charged with the other: for if we heat a mixture of cinnabar and oxide of tin at the maximum, we obtain mercury, ſulphurated oxide of tin, and ſulphureous acid. This laſt is here produced, as in the caſe where ſulphur is treated with the oxide of tin, by a reduction which brings this oxide from the maximum to a minimum of oxidation, of which I do not yet know the numerical value. Tin takes ſulphur from cinnabar; but no oxigen: that principle not being preſent.

Sulphuret of Arſenic.

This is alſo one of thoſe which have long deceived me by the analogy which its tranſparency and colour ſeems to indicate. The ſulphurets of arſenic contain no oxigen.

The arſenical acid and white oxide, treated with ſulphur, loſe their oxigen, yield ſulphureous gas, and are reduced into a ſulphuret, which is tranſparent, vitreous, of a yellowiſh red colour, and capable of being raiſed in diſtillation.

Pure arſenic, treated in the ſame manner, yields the ſame kind of ſulphuret, of perfect volatility, colour, and tranſparency.

In whatever ways we may expoſe theſe ſulphurets to the action of pulveriſed charcoal, iron, tin, &c. they remain unalterable. Minute portions of ſulphureous gas, and ſulphurated and arſenicated hidrogen, appear at firſt in conſequence of the moiſture, but nothing more.

The native, fcaly, flexible orpiment melts quietly, and forms a vitreous, red, tranfparent mafs, refembling realgar, and unalterable by the action of charcoal. There may exift natural fulphurated oxides of arfenic, but certainly thefe are not fuch as are formed by an elevated temperature. The native realgar of Ronda in Andalufia, is alfo a metallic fulphuret without oxigen.

Sulphuret of Copper.

The fulphuret of copper exhibits a fecond inftance in the mineral kingdom of fulphurets with a furcharge of fulphur.

Characters of fulphuret of copper.

As the lights of analyfis have hitherto been wanting, in order to point out to mineralogifts the real characters of this fulphuret, they have always defcribed it in an imperfect manner, or confounded it with other mineral productions. In its pureft ftate, its colour is always a deep blue, violet, or mixed with the copper tinge of indigo, if rubbed with a fmooth fubftance. Its colour is liable to be difguifed by a mixture of carbonate of copper, red oxide of iron, &c. But when we feparate thefe by folution, the fulphuret is reftored to its real colour.

Variations from impurity.

More commonly the fulphuret of copper is difguifed by its intimate combination with the other metallic fulphurets.

Combined, for example, with the fulphuret of iron, it affords the copper-coloured pyrites, in which the exiftence of the blue fulphuret may eafily be demonftrated.

If with thefe two fulphurets, confidered as the bafe, are combined thofe of antimony, lead, arfenic, mercury, filver, zinc, &c. each feparately, there refults a feries of ores of copper, rendered grey by as many different metals, but which do not on that account, as mineralogifts believe, neceffarily contain filver.

Compound ores of copper.

If with the two firft fulphurets we combine in idea the 2d, 3d, 4th, and 5th of thefe which we have juft mentioned, we have in thefe combinations as many complicated ores of copper. Amongft thofe which are brought from America, there are others likewife compounded with native filver, the carbonates of iron, manganefe, fulphate of barytes, &c. Such are the different fpecies of mineralizations which mineralogifts have indifcriminately comprehended under the two claffes of white and grey ores of copper.

The

The grey ore of copper of La Cren, in the kingdom of Valentia, is of this kind: it is composed of four sulphurets, that of copper, of iron, of antimony, and of mercury. Deposited in masses in the calcareous breccias formed by the remains of mountains which no longer exist, it undergoes a decomposition at its surface, which converts three of the sulphurets into oxides, whilst that of mercury remains entire, and imparts to the decomposed ore a brilliant vermilion hue. At the centre there is always a nucleus composed of these four sulphurets in their entire state. The analysis of this mineral, given by Fernandez, is in the *Annales*: but I return to the sulphuret of copper, from which I did not intend to digress.

Copper pyrites, like that of iron, contains an excess beyond the state of sulphuret.

Those which I have had opportunity to examine contain from 14 to 15 per cent. of sulphur, which is easily extracted from them by a moderate heat, and what remains in the retort is always the blue sulphuret, saturated according to an uniform proportion. This proportion is 18 per cent. like that of the artificial sulphuret. If a native sulphuret of copper leaves 86 of residuum, we may precipitate its nitric solution by sulphurated hidrogen; and the precipitate separated from the excess of sulphur re-produces 86 parts of sulphuret of copper. Hence we see, that though the native sulphuret is subject to an excess of sulphur, it does not on that account differ from the artificial sulphuret, when it has been deprived of this excess.

Copper sulphurets contain no oxigen.

By the action of a red heat, the oxides of copper, mixed with sulphur, yield only blue sulphuret. This metal does not afford a sulphurated oxide, either in nature or in the products of art. Its formation is always attended with a considerable extrication of heat. It is astonishing, if it be duly considered, that it could ever for one moment have been mistaken for a combustion.

Black copper.

The blue sulphuret dissolves in copper, and forms the black coppers, independently of the iron which they may contain. Acids of 10 or 12 degrees separate it from these coppers without decomposing it.

On the Fuming Muriate of Tin.

Fuming muriate of tin prepared without amalgamation.

Since the year 1777, I have been in the habit of preparing this muriate with corrosive sublimate and pulverised tin; and though I never thought of publishing this process, it was because I imagined it to be in common use in every laboratory. But

But as, in the lateſt treatiſes of chemiſtry, the ancient proceſs of amalgamation is ſtill preſcribed, I think it may be uſeful at preſent to recommend the uſe of the mere pulveriſed tin.

Theſe are the details which I find in my notes of that period; they ſhew that the following proportion is the beſt:

Inſtructions.

Twenty-four ounces of ſublimate and eight ounces of tin yield nine ounces of fuming liquid.

Conceiving that ſome advantage might be obtained by augmenting the proportion of ſublimate, on account of the great exceſs of pure and ſemi-oxided tin which is found in the reſiduum, I made the following trial:

Experiment.

Thirty-two ounces of ſublimate and eight ounces of tin yielded ten ounces of the liquid; hence it appears, that the expenditure of eight ounces of ſublimate is not compenſated by more than one ounce more of liquid.

The acid is not oxided in the fuming muriate.

I ſhall ſhew, upon another occaſion, that in the fuming muriate of tin, as well as in the corroſive muriate of mercury, the baſes only are oxidated at their maximum, whilſt the muriatic acid remains in its ordinary ſimple ſtate.

VIII.

A Memoir on the Method of Bleaching the Paſte of Paper. By CITIZEN LOYSEL *.

Berthollet's bleaching proceſs.

THE advantages of the method of Citizen Berthollet for bleaching thread and piece goods, by means of the oxigenated muriatic acid are univerſally known. Citizen Chaptal has uſefully applied this invention, to reſtore the colour of prints and printed books; and he has alſo ſimplified one of its moſt important parts in his new proceſſes reſpecting the art of applying the lixivium.

Advantages of applying it to paper.

The adoption of Citizen Berthollet's method to the art of paper-making, is capable of carrying this branch of induſtry to a ſtill greater degree of perfection, particularly in France. We are abundantly ſupplied with the raw materials proper for manufacturing paper; but according to the preſent proceſſes of our paper mills, there is only a very ſmall portion of the rags

* Tranſlated from the Annales de Chimie XXXIX. 137.

rags which can be uſed to manufacture fine white paper. All the reſidue is condemned to the fabrication of papers of inferior quality.

The proceſs of bleaching the paſte of the paper maker, even when produced from the moſt common rags, will communicate to it the quality of the beſt ſort. By theſe means our paper manufactories may ſupply our wants in fine white paper, and even obtain the preference in foreign markets. The reſult of this operation would be, that a greater number of workmen would find employment, and the advantages of this increaſe of induſtry would be of ſtill greater national value, than even the foreign export which might be expected.

Actual trial and practice. The ſucceſs of bleaching the paſte of paper by the method of Citizen Berthollet is no longer problematical. The application which has been made to the paper uſed in making aſſignats, has placed this queſtion beyond all doubt as to its ſolution.

In making paper for aſſignats. It was at the commencement of the year 2, that the committee of aſſignats and monies of the National Convention, of which I was a member, reſolved to employ this method, together with that of ſtereotypage which had been adopted, to oppoſe new obſtacles to the practice of forgery.

We particularly conſulted Citizens Berthollet, Fourcroy, and Guyton on this enterprize. Their approbation of the project, and the information they afforded us, ſoon gave us the power of realiſing it. We were alſo aſſiſted with the knowledge of Citizens Welter, Athenas, Alban, Carny, Marchais and Ribaucour, who with great zeal communicated their proceſſes, and permitted us to inſpect their ſeveral manufactories.

Order of narration. I ſhall not here relate all the previous experiments we made before we eſtabliſhed our works on a large ſcale, but ſhall content myſelf to point out conciſely the manner of our commencement, the agents we employed, and the means which to us appeared the moſt ſimple to obtain our deſired purpoſe. I ſhall afterwards add a few obſervations reſpecting the means of ſtill greater perfection, which the advancement of the ſcience ſince that time has rendered the ſame operations capable of.

The rags were bleached at the firſt trial. Our firſt proceſſes were executed preciſely according to the method of Citizen Berthollet. The rag was ſubjected in

fucceffion to different lees, to baths of the bleaching liquor, and fulphuric acid pointed out in his memoir. Berthollet had fhewn, and we were alfo convinced by our own experience, that the gas is lefs confined in the fimple fluid, prepared without addition of fixed alkali, than it is in that which contains potafh or foda; and that it is confequently more difpofed to feparate and enter into new combinations. We therefore at firft made ufe of this fimple liquor; but the workmen foon exhibited a ftrong repugnance to its ufe on account of the fumes it emits, which are extremely inconvenient, even when chalk is diffufed in the liquor. This inconvenience forced us to abandon it, though with regret. This facrifice was fo much the more confiderable, as it occafioned a lofs of time, and confiderable increafe of expence. We decided that we would receive the gas in a folution of potafh; but as the dofes in which this alkali may be ufed have limits of great extent, we endeavoured to keep as near as poffible to that preparation which is fufficient to prevent the fpontaneous difengagement of the gas, and by that means caufe the liquor to lofe the odour we were defirous of avoiding. This dofe was 5 kilograms of potafh to 100 litres of water, (11 pounds avoirdupoife, to 21½ ale gallons.)

Difficulty of ufing the bleaching liquor without alkali.

The rags bleached in this manner became of the moft brilliant white. Neverthelefs, a part of this perfection difappeared, when the rag was converted into pafte, and that pafte into paper. Is was eafy to difcover the caufe; namely, that the interior parts of the thread in the rag were lefs expofed to the action of the liquor than thofe at the furface. This motive determined us to abandon the bleaching of the rags, and to operate upon the pafte itfelf.

The rags became very white, but when opened by the mill were lefs fo.

We were here oppofed by new obftacles. When the rag is converted into a pafte proper to be worked, its coherence is fuch that it fettles, and no longer permits the lees and baths of the bleaching liquor to penetrate through all its parts, in confequence of which property the paper was found to have veins and different fhades of colour. We remedied this inconvenience, by taking the matter in a mean ftate between the rag and the pafte proper to be converted into fheets of paper. We fucceeded in this refpect by deftroying the texture of the rag under the firft cylinder fo as to feparate its fibres, an operation which ufually lafted two hours for a pile of 50 kilograms.

The pafte was therefore bleached in its half prepared ftate.

Thus

Thus it was, that by ſucceſſively avoiding the extremes of too much and too little mechanical connection, we advanced towards our object.

The apparatus which Citizen Welter imagined, and of which Citizen Berthollet has given a deſcription in the firſt volume of the Journal of Arts and Manufactures, is applicable to all the methods which can be employed to procure the different kinds of bleaching liquor, whether the water of the receiver contain fixed alkali or not; whether the muriatic acid be uſed on the oxide of manganeſe, or the gas be obtained by ſulphuric acid, upon the mixture of oxide of manganeſe and muriate of ſoda. This apparatus is particularly preferable to all others in the caſe where the water of the receiver contains no alkali, becauſe the abſorption of the gas is favoured by its being brought into contact with the water at a great number of ſurfaces. But as we had determined to uſe a ſolution of potaſh, we were able to make ſome modifications of this apparatus. Apparatus of Welter for ox. mur. acid

1. The three inner partitions of the receiving veſſel were reduced to one only. Ours was formed of a tub containing another tub inverted, and both were covered with ſheet lead. was conſiderably ſimplified.

2. The ſize of the tubes of communication was ſo conſiderable, that we had nothing to fear from abſorption during the courſe of the diſtillation.

3. Laſtly, the practical ſkill of our workmen in managing the fire, and the advantage of having only one opening to lute enabled us to ſuppreſs the intermediate veſſel.

Deſcription of the apparatus uſed in preparing the oxigenated muriatic acid in bleaching the paſte of paper for aſſignats. Deſcription.

Fig. 1, Pl. VII. Plan of the apparatus. Plan.

1, 1, 1, &c. Eight furnaces, having a chimney of ſheet iron common to each pair of furnaces.

2, 2, 2, &c. Eight veſſels of caſt iron, containing ſand.

3, 3, 3, &c. Eight matraſſes, balloons, or bottles of ſtone ware, compact and well baked, intended to contain the materials which afford the gas. Each matraſs muſt be filled only to one-third of its capacity at moſt. Bodies of glaſs of little thickneſs may alſo be uſed for this purpoſe.

4, 4, 4, &c. Tubes of glaſs to conduct the gas into the receiver. Or theſe tubes may be made of lead.

5. The

Plan.

5. The receiver. It is compofed 1. of an external veffel, covered with plates of lead well foldered together, and provided near its bottom with a cock 6, to draw off the liquor when prepared. 2. Another veffel, 7, likewife covered with plates of lead within and without. This fecond tub is inverted in the firft to contain the gas in proportion as it is difengaged, and to keep in contact with the water of the receiver, that portion of gas which had not time to be diffolved, in paffing through that fluid.

There is a hole, 8, in the upper part of this fecond veffel, It ferves to fuffer the common air to efcape when water is firft poured into this receiver, and it is afterwards clofed with a ftopper of lead or cork, covered with paper, foaked in ftarch, and faftened to the cork by a piece of cloth or bladder, before the operation began.

Vertical fection.

Fig. 2. Vertical fection of the apparatus.

Fig. 3. Elevation of the apparatus.

The difpofition of the furnaces about the receiver, and the circular form of the receiving veffels, was rendered neceffary by the local circumftances of the laboratory in which our operations were carried on. In other circumftances fquare veffels might be employed, and all the furnaces might be ranged in a right line under a common chimney.

One thoufand litres of water are placed in the receiver, holding in folution fifty kilograms of white purified and calcined potafh.

When the difengagement of gas is effected by the muriatic acid, the materials are ufed in the following dofes:

Oxide of manganefe - - - - - -	24 kilograms
Muriatic acid at 20 degrees of denfity according to the earometer of Baumé,	68
	—
	92

which makes for each of the eight diftilling veffels $11\frac{1}{2}$ kilograms of materials.

Charging and diftillation.

The operation is begun by charging the receiver with 1000 litres of alkaline water, after which the aperture 8 is clofed with its ftopper well luted. Each mattrafs is then placed in its fand-bed; and pulverized manganefe is introduced. The muriatic acid is poured upon the manganefe, and the ftoppers into which the tubes of communication pafs, are duly placed. The juncture is luted with paper foaked in ftarch,

ſtarch. And the lute is left to dry from ſix to twelve hours, after which the fire is lighted in the furnaces.

The proceſs of diſtillation laſts from ten to twelve hours. When it is finiſhed the tubes are unluted, the fire extinguiſhed, and the mattraſes ſuffered to cool in their ſand beds, till the temperature of theſe beds has deſcended to 60 or 70 degrees, (centigrade) at which period, water of the ſame heat is poured into the mattraſes. The water dilutes the reſidue of the diſtillation, which mixture is to be poured out, and the veſſels ſuffered to cool in baſkets containing ſtraw. If the precaution of introducing hot water in this manner upon the reſidue were not taken, it would become ſo ſolid when the operation is performed with ſulphuric acid, in the manner we are about to deſcribe, that it could not be extracted without much trouble and danger of breaking the veſſels.

If the diſengagement of the gas be made by ſulphuric acid, the following doſes are uſed: Proceſs with ſulphuric acid.

Oxide of mangaueſe - - - -	25 kilograms
Muriate of ſoda - - - - - -	70
Sulphuric acid at 50 degrees of denſity	25
Total	120

The acid is to be diluted with an equal bulk of water, or 16 litres, which will reduce its denſity to 31 degrees.

The eighth part of this for each mattraſs amounts to 14$\frac{1}{8}$ kilograms.

The oxide of manganeſe and muriate of ſoda being pulverized are mixed together. The mattraſs is to be charged and the operation conducted as before deſcribed. This method is the moſt economical, becauſe the ſulphuric acid is cheaper than the muriatic, and alſo becauſe it is practicable to obtain from the reſidue of the diſtillation, the ſoda of the muriate which is converted into ſulphate of ſoda; that ſalt being decompoſable by well known proceſſes.

In order to meaſure the force of theſe liquors, or their bleaching power, we made uſe of the ſolution of indigo preſcribed by Citizen Deſ roizilles, which is prepared in the following manner: To aſcertain the ſtrength of the bleaching liquor

In a glaſs mattraſs put of concentrated ſulphuric acid at 66 degrees of denſity, ſeven parts by weight, and of pulverized indigo one part; agitate the mixture, and plunge the ball of the By indigo.

the mattrafs to half its depth in water moderately heated, agitating it from time to time. The folution will be effected in two hours, after which it is to be diluted in 992 parts of water. This is the proof liquor. The ftronger the bleaching liquor, the greater number of parts of the folution of indigo it will render colourlefs, and we may by this proof determine the dofes of each kind of bleaching liquor to be employed, together with water, fo as to compofe a bath proper for immerfing the fubftance intended to be bleached.

One part by meafure of the bleaching liquid prepared as before mentioned, will ufually deftroy the blue colour of nine parts of the proof folution of indigo; it was of the fame ftrength as that of Javel, prepared by Citizen Alban.

Choice and Preparation of the Rags.

Selection of the materials for paper, quality of rags, &c.

The ftrength or tenacity of paper depends upon the ftaple or fibre of the material from which it is made. Rags of new cloth and cordage compofe a paper more tough than old rags, and the firft of thefe materials prefents a great variety on account of the quality of the hemp or flax of which they are formed. Rags of fine new cloth, whether raw or bleached by the oxigenated muriatic acid, ftand in the firft rank, after which cordage and old rags may be claffed.

Bank or note paper.

Paper intended for bills of exchange, or other commercial and legal inftruments ought to be tough, in order that it may not be eafily torn when thin, for this paper the materials of the firft clafs muft be intirely, or in large proportion employed. The price which the confumers are difpofed to pey for this article, is fufficient to indemnify the manufacturer for his care and induftry, as this kind of paper is fold in France for 5 or 6 francs the kilogram.

Common paper.

The other papers alfo require to be more or lefs tough, according to their thinnefs, and the ufe to which they are applied, but a clear white colour is fought in paper of every defcription. The firft operation to which the rags are fubjected is forting, in order that each branch of the manufacture may have its appropriate material, after which they are cut with fhears into pieces of about one decimeter, or three or four inches fquare.

I will fuppofe that the object of the manufacturer is to obtain paper of a beautiful white. If it is intended to be thin, fo

that,

that, for example, a ream of the ſize denominated *raiſin* ſhould weigh only four or five kilograms, that is to ſay, about one-third of the weight of common paper of the ſame form. The manufacturer makes choice either of new rags already of a fine white, or of unbleached rags.

Treatment of the rags, &c.

In the caſe of the white rags, it is ſufficient to paſs them under the firſt cylinder, then to give them a bath of the bleaching liquor, and afterwards a bath of ſulphuric acid, as we ſhall proceed to direct, after which they are paſſed under the finiſhing cylinder for ſeven or eight hours, and, laſtly, conveyed to the working trough to be made into ſheets of paper.

Rags, which have never been bleached, may be treated by either of the following proceſſes, that is to ſay, the firſt, which preſerves the utmoſt degree of toughneſs to the paper, but is likewiſe the moſt expenſive, conſiſts in decompoſing the rag, and afterwards applying the method of Citizen Berthollet for bleaching piece goods; namely, ſubjecting it to three or four lixiviations, and afterwards alternately to lixiviations, baths of the bleaching liquor, and baths of ſulphuric acid. The weight of the raw unbleached material is diminiſhed from 50 to 45 per cent. in theſe operations.

Great advantage of ſuffering the rags to undergo the ſpontaneous change of rotting to a certain degree.

This method was the firſt which we uſed for the aſſignat paper; but we ſoon perceived that we might omit moſt of the lixiviations and baths of the bleaching fluid, and ſtill preſerve as much toughneſs as the paper required. Nothing further was neceſſary for this purpoſe than to ſuffer the rag to undergo a degree of fermentation more or leſs advanced, by leaving it to rot. In this operation the colouring matter undergoes a ſlow combuſtion, and paſſes to a kind of ſaponaceous ſtate, and is carried off by the water, by waſhing the rags in the veſſel of the firſt cylinder.

The bleaching is greatly facilitated.

One ſingle lixiviation, two baths of the bleaching liquor, and one of ſulphuric acid, are then ſufficient to bleach completely the raw rags or cordage. This is the ſecond method. We were not, at that time, acquainted with the economical proceſs of Citizen Chaptal in the operations of lixiviation. This will, no doubt, be uſed; but the effect of rotting, carefully conducted, will always he found very advantageous.

Laſtly, if the rags be neither perfectly white nor raw and unbleached, but in a medium ſtate, that they are left to rot for a ſhorter time, for example, twelve or fourteen days, and are taken up when the heat of the fermentation raiſes the thermometer to 30 or 35 degrees, after which the proceſs is to be conducted as before mentioned.

Compoſition of a bath of the bleaching liquor, for a pile of decompoſed rags, weighing 50 kilograms.

Application and proportions of materials in the actual work.

For each heap of rags a certain number, for example, eight or nine, wooden tubs are diſpoſed in a line, capable of containing in the whole 600 litres of water: 450 litres of pure water is poured in, and 90 litres of bleaching liquor are added in equal portions to each of the veſſels, after which the 50 kilograms of decompoſed rags are diſpoſed in equal portions in each tub. The ſtuff is left for about twelve hours in this bath, agitating it from time to time, after which it is to be completely waſhed in clean water, and put into a bath of ſulphuric acid, compoſed of water 200 litres, and acid at 50 degrees three kilograms, which bath will then have the ſtrength of about four degrees of the earometer of Baumé.

The immerſion in the bath muſt continue for three quarters of an hour or an hour, after which the materials muſt be well waſhed in clear water, and carried to the mill to be manufactured.

If the action of the baths of bleaching liquor be not exhauſted by the immerſion of the decompoſed rags (which may be aſcertained by the ſolution of indigo), it may be applied to other materials of the ſame kind.

Improvements.

Such was the ſtate in which we left this new art in the year 3. Since that time Citizen Welter, to whom chemiſtry and the arts are indebted for a number of ingenious proceſſes, has ſimplified that of preparing the bleaching liquor. He has found, for example, that inſtead of the three veſſels of the receiver, it is ſufficient to employ two even for the ſimple liquor that contains no fixed alkali.

Riſe of the liquor without alkali.

It was before ſeen that we were obliged to employ an alkaline ſolution in the receiver, to prevent that odour which the ſimple liquor emits when paper ſtuff is agitated in the baths. The uſe of alkali anſwered our purpoſe very well in this reſpect;

ſpect; but this expenditure, beſides weakening the bleaching liquor, nearly doubled our expence. Though this difference in the price was of little conſequence with regard to the object we then had in view, it is not ſo with regard to the common operation upon paper intended for ſale. Every means of economy muſt then be uſed. Now Citizen Welter found that it is eaſy to obviate the inconvenience of the ſimple liquor in the operation. His method conſiſts in no longer agitating the goods or material in an open bath, but to cloſe it exactly by means of a cover; and he agitates it by means of croſs pieces attached to a handle turned on the outſide.

Eſtimate of charges.

A rough eſtimate of the price of the ſimple bleaching liquor prepared by the ſulphuric acid, this being the moſt economical proceſs.

The receiver is ſuppoſed to contain 1000 litres of water.

25 kilograms of oxide of manganeſe coſt at moſt - - -	15fr.	0c.
70 kilograms of muriate of ſoda -	7fr.	0c.
25 kilograms of ſulphuric acid, at 50 deg.	37fr.	50c.
Three days work, principal men -	9fr.	0c.
Three days ——, aſſiſtant or labourer	4fr.	50c.
Fuel, about - - -	3fr.	
Wear and tear - - -	6fr.	
Our apparatus coſt 622 franks, and the carriage and fixing increaſed our expence to 1000 franks, the intereſt of which, at 10 per cent. is 100 franks; and if the work be repeated ſo many times in the year, the intereſt per operation will be - - -	1fr.	
	83fr.	

Hence the litre of bleaching liquor will coſt nearly 9 cents in round numbers. *

* As the price of all theſe ſeveral items in France muſt materially differ from the ſame in England, I have thought it unneceſſary to reduce the numbers.

Eſtimate

Eſtimate of the increaſe of expence occaſioned in the operation upon a pile of 50 kilograms of the paſte of paper, ſuppoſing one bath of the bleaching liquor and one of ſulphuric acid, which is moſt commonly the caſe.

Ninety litres of the bleaching liquor at nine cents.		9c.
	8fr.	10c.
Three kilograms of ſulphuric acid, at 1fr. 50c. - - -	4fr.	50c.
Workmanſhip - - -		50c.
Total	13fr.	10c.

Which gives for each kilogram of paper an expence of 0,262 franks, or about 27 cents. Now the common paper in the market uſually ſells for about 1 fr. 30c. or 1fr. 40c. the kilogram, and with the ſimple augmentation of 27 cents for the operations of bleaching, it obtains the preference beyond that which is ſold for three, four, or even five franks, which can only be obtained in limited quantity, on account of the ſelection of rags. The foregoing methods muſt therefore produce a great diminution in the price of fine paper. They are more particularly advantageous when applied to the manufacture of thin paper, becauſe the expences of bleaching are always proportioned to the weight of the material, and conſequently are leaſt upon thin paper.

Diſtilling apparatus of *Athenas.*

I ſhall conclude this memoir with a deſcription of an apparatus, invented by Citizen Athenas, for preparing the oxigenated muriatic acid. He had the complaiſance to lend it to us. This apparatus is remarkable for its ſimplicity, as it requires no veſſels of earth or glaſs, nor intermediate veſſels, nor tubes of communication.

Fig. 4, Plate VII. repreſents the horizontal ſection, and Fig. 5 the vertical ſection. The deſcription of this laſt will be ſufficient to explain the conſtruction.

9 repreſents the aſh-hole.

10 the fire-place, having its chimney turned on one ſide.

11. An iron boiler, containing water kept at the heat of ebullition, ſerving as a bath for the cucurbit, and effecting the diſengagement of the gas.

12. The diſtilling veſſel of lead, in which the ſubject matters of the operation are put.

13. A

13. A veſſel lined with lead, interſected by the veſſel 12, with which it is ſoldered.

14. A cock to draw off the fluid when prepared.

15. A receiver of lead inverted in the tub. A hole, 16, may be made in this receiver to let out the atmoſpheric air when it is plunged in the tub, which hole muſt be cloſed before the diſengagement of the gas begins. The inverted receiver bears upon three projections of wood, which keep it at a little diſtance above the bottom of the tub.

When the ſtrength of the liquor obtained by this apparatus was compared with ours, it was found that with equal proportions of materials and of water its diſcolouring action upon indigo was no more than about the half of ours. Conſequently there is a real loſs in the conſumption of theſe articles. This muſt be attributed to the low degree of heat, which does not expel the whole of the gas. I have nevertheleſs judged that it would be of uſe to deſcribe this ingenious mechaniſm, the ſimplicity of which will, no doubt, give ſatisfaction to artiſts. It is obviouſly capable of improvement, either by placing on one ſide a ſeparate furnace, with a ſand and ordinary diſtilling veſſel, whence the gas may be conducted beneath the receiver by a leaden tube; or, according to the actual diſpoſition of the apparatus, a liquid or dry bath may be ſubſtituted to that of water, ſo as to communicate a higher degree of heat, provided the temperature be leſs than will melt lead, a condition which admits the deſired extrication of the gas.

This laſt apparatus was leſs effective than the former.

But it may be improved by raiſing the heat, and is very ſimple.

IX.

On the Sound produced by a Current of Hidrogen Gas paſſing through a Tube. With a Letter from Dr.* HIGGINS, *reſpecting the Time of its Diſcovery.*

INTO a glaſs bottle is put ſulphuric acid and iron filings, through the cork of which a glaſs tube is paſſed; the upper extremity is capillary: then by ſetting fire to the hidrogen gas which eſcapes by this extremity, a continued current or jet of

The flame of hidrogen paſſing through a capillary aperture, under a tube,

* Bulletin des Sciences, No. 56, An. 10.

of flame is produced, which is let paſs into a tube of either glaſs, metal, earth, or any other ſubſtance; when the following phenomena are obſerved.

produces clear ſounds.

If the tube be not too large, the flame becomes ſmaller as it is depreſſed, and when it is much reduced, the tube emits very clear ſounds.

But if the tube be too narrow, the flame will be extinguiſhed, and in proportion as the tube is larger, the ſound diminiſhes; ſo that there is a certain limit at which it totally ceaſes, which alſo happens when the tube is too long.

The ſounds may be varied at pleaſure, by either uſing tubes of different figures and dimenſions, or made of various ſubſtances.

This experiment made in Italy;

Theſe experiments have been made in Italy. Citizen Brugnatelli had deſcribed them in his Annals of Chemiſtry; and he has repeated them with Citizen Volta in the cabinet of the Polytechnic School, in the preſence of ſeveral perſons.

LETTER *from* Dr. HIGGINS.

To Mr. NICHOLSON.

DEAR SIR,

but was made 25 years ago by Dr. Higgins.

IT was in the year 1777, that I firſt exhibited the experiment you mention, and produced ſeveral ſweet tones, according to the width, length, and thickneſs of the glaſs jar or ſealed tube, held by the bottom, inverted to ſome depth over the flame of hidrogen gas. This experiment was afterwards ſhewn by ſome of my pupils, to amuſe their friends, and particularly by my couſin, Mr. Higgins, who now teaches in Dublin; who exhibited it at Sir Joſeph Banks's, and, if I am not miſinformed, at Oxford, where he ſerved the profeſſor as operator. *

Hiſtory of the diſcovery.

Theſe tones firſt occurred unexpectedly, whilſt I was making this eaſy and popular experiment, to ſhew the water formed during the ſlow combuſtion of the ſlender ſtream of hidrogen gas in the air of the jar. And as I always had a

* I remember the experiment at Mr. Kirwan's, in 1784, which led me to requeſt the information here given.

great

great variety of fuch jars on the table, many different tones were produced on the very firft day, by applying jars of different capacity and thicknefs.

I am, SIR,

Your's refpectfully,

B. HIGGINS.

Percy Hotel, Jan. 12, 1802.

The caufe of the curious phenomenon here defcribed feems difficult to be clearly afcertained. As it is univerfally admitted that found is caufed by undulations, that is to fay, alternate condenfations and rarefactions of the air, or fome more fubtle fluid, we are naturally led to an intenfion and remiffion of the flame of the hidrogen. It is probable that all flame undulates. The flame of a candle is faid to dance when this effect is moft remarkably evident. I fuppofe the fupply of oily vapor enlarges the flame, until its furface becomes fo extenfive, as to abforb oxigen in greater quantity than the fuel can be fupplied to keep up a combuftion of that fize: it therefore becomes lefs, until by the diminution of furface, the due proportion of oily vapor, compared with the oxigen, again exceeds. And in this way we may form a notion of perpetual undulations of fize. If thefe changes were rapid and ftrong enough, the air would be ftruck by them into fonorous undulations. In the fame manner perhaps we may account for the loud chirping noife I have obferved to be produced by the inflamed drops of tallow which fall from a candle held nearly upfide down at five or fix feet from the ground.

Speculation on its caufe.

Caufe why the flame of a candle dances.

A chirping noife produced by flame.

W. N.

X.—*Defcription*

X.

Description of the Graphometer, or Instrument of CIT. CARANGEAU, *for measuring the Angles of Crystals.—From the Mineralogie of* CIT. HAUY *.

Structure of the graphometer. Two concentric, equal quadrantal arcs joined by an hinge; to which are applied compass legs or radii, capable of being shortened to apply to small crystals, &c.

IN order to determine the mutual inclination of the faces of a crystal, or its prominent angles, an instrument is employed, for the invention of which we are indebted to Citizen Carangeau. This instrument, which considerably resembles the graphometer, consists of a semicircle M T N, (Pl. VIII. fig. 1.) of brass or silver, divided into degrees, to which two indexes, A B, and F G, are applied, the one of which, F G, is hollowed out from *u* to R, from face to face, except at the place K, where a small portion is left for the purpose of giving greater strength to the instrument. This index is attached at R and *c* to a brass piece or rule situated behind, and forming the same piece with the limb. The connection of the index with this rule is effected by means of two small milled-head-screws, inserted in the groove. The other index, A B, is hollowed out in the same manner from *x* to *c*, where it is attached above the former, by means of the screw at the center, which traverses the two grooves. By loosening the screws, we may shorten at pleasure the parts *c* G and *c* B of the two indexes, accordingly as circumstances may require.

The index A B having only one point of attachment at *c*, at the center of the circle, moves round this circle, whilst the index G F remains constantly in the direction of the diameter, which passes through the points zero and 180 degrees.

It may be proper to remark, that the upper part of the index A B ought to be sloped to an edge, on its side *s z*, the direction of which edge, if prolonged, would pass through the center *c* of the instrument. The reason is, that this side is the fiducial edge; that is to say, it indicates upon the graduated circumference the dimensions of the angle we wish to measure.

Mode of operation.

Let us now suppose that we are desirous of measuring in any crystal, the angle which two contiguous planes form with each

* This ingenious instrument has been in use a considerable time, and deserves to be more generally known. W. N.

each other. It is known that this angle is equal to that of two lines drawn to any ſingle point of the edge which unites theſe planes, provided they be perpendicular with this edge, and ſituated in the planes themſelves. To find this angle, we diſpoſe the inſtrument in ſuch a manner, that the portions *c* G and *c* B of the two indexes ſhall have no ſpace between themſelves and the points in queſtion, and that their ſides ſhall be perpendicular to the edge at which they join. In this caſe, the faces which embrace the cryſtal are tangents to the two planes whoſe incidence we wiſh to meaſure. This being done, we look on the circumference of the inſtrument for the degree marked by the fiducial edge *s z*, or the angle which this line forms with that which paſſes through the center *c* and the point zero, which angle is equal to that formed by the two portions G *c* and *c* B of the indexes, becauſe vertically oppoſite.

Advantage of ſhortening the legs.

It is an advantage to be able to ſhorten theſe parts at pleaſure, in order to avoid the obſtacles which would otherwiſe render the operation impracticable, either from the gangue to which the cryſtal adheres, or from the contiguous cryſtals by which it is partly ſurrounded.

In ſome cryſtals this expedient is not ſufficient;

Caſes, however, occur in which this expedient would not be ſufficient, and we ſhould find ourſelves embarraſſed by the part of the ſemicircle ſituated towards M, if its poſition were invariable. The ingenious inventor of the inſtrument has guarded againſt this inconvenience by means of the following mechaniſm:

but part of the circular limb muſt be occaſionally removed.

The ſcrew ſituated at *c* holds not only the two indexes, but alſo a ſteel radius placed below the braſs arm, upon which the index G F is immediately applied. The upper extremity of this radius, or that which is ſituated towards O, has an excavation into which a braſs ſtem enters, which is likewiſe provided with a ſcrew. The ſemicircle is beſides divided into two pieces, at the place of the 90th degree; ſo that, by means of a hinge at that part, the quadrant T M may be folded behind the quadrant T N, and is, as it were, ſuppreſſed. When we wiſh to perform this movement, we looſen the ſcrew which ſupported the upper part of the radius *c* O, diſengage the excavation which terminates this radius from the ſcrew which was inſerted into it, and draw back the radius till it lies beneath the braſs arm which ſupports the index G F.

When

When the angle which we meafure exceeds 90 degrees, we return the quadrantal arc T M to its former fituation, in order to afcertain its dimenfions.

The precife meafure of angles completes the defcription of a cryftal.

The utility of the goniometer will be very apparent to every one who confiders how interefting it is in defcriptions of cryftals, to know the angles which their faces form with each other. Thefe indications render the defcription appropriate, by diftinctive and truly characteriftic traits: without them it can be nothing more than a rude and imperfect fketch, that may apply to a variety of different objects.

Without this they cannot be diftinguifhed.

Inftances.

Thus we do not give a determinate notion of the dodecahedral zircon, when we merely fay that it is a prifm of four fides, terminated by fummits with four rhombufes attached to the longitudinal edges. This defcription applies equally to the Harmotome (cruciform hyacinth) to the ftilbite, to oxided tin, &c.; but if we add, that the fides form right angles with each other, and that the faces of the fummit are inclined towards each other in an angle of 124° 12′, we confine the defcription to the zircon. If we fay that the inclination is 121° 57′, we defcribe the harmotome; if we fay that there are two different inclinations, the one of 123° 32′, and the other of 112° 14′, we defcribe the ftilbite.

Other cafes.

And ftill farther: Several varieties of the fame fubftance may prefent fimilar forms, differing only in the meafures of their angles. Such are, on the one hand, the fix rhomboides, and on the other the two decahedrons with rhombic faces, which are found in the carbonates of lime. How fhall we defcribe with accuracy all thofe varieties, which differ only in the more or lefs, unlefs we give the differences with precifion? There are even cafes in which the ufe of the goniometer is the only means of avoiding an error, which otherwife would not fail to infinuate itfelf into the defcription. Thus the calcareous rhomboid, the angles of which differ only by about 2° 18′ from the right angle, was at firft taken for a cube, and we fhould have continued to denominate it cubic calcareous fpar, if geometrical meafurement had not rectified this denomination, which is faulty in two refpects, both in itfelf, and with reference to the theory, which demonftrates that the exiftence of the cube cannot in this inftance be reconciled with any of the fymetric laws of diminution.

XI.—*Letter*

XI.

Letter of Professor VOLTA *to* J. C. DELAMETHRIE, *on the Galvanic Phenomena.*

SIR, *Paris,* 18 *Vendemiaire, Year* 10.

YOU have requested me to give you an account of the experiments by which I demonstrate, in a convincing manner, what I have always maintained, namely, that the pretended agent, or *galvanic fluid,* is nothing but common electrical *fluid,* and that this fluid is incited and moved by the simple *mutual contact of different conductors,* particularly the metallic; shewing that two metals of different kinds, connected together, produce already a small quantity of true electricity, the force and kind of which I have determined; that the effects of my new apparatus (which might be termed electromotors), whether consisting of a pile, or in a row of glasses, which have so much excited the attention of philosophers, chemists, and physicians; that these so powerful and marvellous effects are absolutely no more than the sum total of the effects of a series of several similar metallic couples or pairs; and that the chemical phenomena themselves, which are obtained by them, of the decomposition of water and other liquids, the oxidation of metals, &c. are secondary effects; effects, I mean, of this electricity, of this continual current of electrical fluid, which, by the above-mentioned action of the connected metals, establishes itself as soon as we form a communication between the two extremities of the apparatus, by means of a conducting bow; and when once established, maintains itself, and continues as long as the circuit remains interrupted. You have requested this sketch from me, to be inserted in the next number of your Journal de Physique, convinced in your own mind of the truth of these observations by some of the experiments which I had the pleasure of shewing you yesterday with my small portable apparatus, in presence of the celebrated philosopher of Geneva, M. Pictet, and some other friends. I regret that I have not sufficient time to enlarge the essay which I send you, so as in some measure to comply with your invitation, and which can answer your expectations only in a partial manner. Accept it then as the precursor of a more ample memoir, which I intend shortly to publish.

General outline of the doctrine of the author respecting galvanism.

That the electrical effects are the primary causes of the oxidation, &c. &c.

I

Narrative of experiments.

I began by ſhewing you, by means of experiments, delicate indeed but yet ſimple, that phenomena unequivocally electrical are produced by the mere contact of two different metals, without the intervention of any wet ſubſtance—experiments which ought to be conſidered as fundamental.

Electrometer and condenſers.

In order to render this electricity (which is ſo feeble that without other artifices it would remain imperceptible) ſenſible and manifeſt, I employ my electrometers with fine ſtraws, combined with my condenſers, the beſt of which are thoſe made with two metallic diſcs, which apply very exactly to each other by their faces, which are very flat, and covered with a ſlight layer of ſealing wax, or, which is better, of good lac varniſh.

Inſulated diſcs of copper and zinc are applied together.

The firſt method of performing this experiment was to take two other diſcs or plates, the one of copper and the other of zinc; to hold each by an inſulating handle (of glaſs covered with ſealing wax); to apply them for an inſtant to each other by their flat faces, and afterwards ſeparating them dextrouſly, to bring them into contact with the electrometer, which then indicated, by the divergence of its ſtraws, to the diſtance of about a line from each other, the electricity which each of the plates had acquired, and whether the electricity was poſitive (or el. +) in the zinc, and negative (or el. —) in the copper, as could be ſhewn by approaching a ſtick of ſealing wax, that had been rubbed, to the ſame electrometer.

On the ſeparation, the zinc was el. +, and the copper el. —.

These plates not only *move* the electricity, but *condenſe* it.

It is proper to obſerve in this experiment, that the two plates, at the ſame time that they are *motors of electricity* by virtue of their mutual contact, as different metals, perform alſo the function of condenſers, becauſe they are preſented the one to the other by a large ſurface, in conſequence of which their contrary electricities are counterbalanced in the beſt poſſible manner. This is the reaſon why this poſitive electricity in the plate of zinc, and negative in that of copper, which otherwiſe would not riſe to more than about a ſixteenth of a degree, and which in fact does not riſe higher as long as theſe ſame plates remain applied the one to the other, elevates itſelf, on detaching them, to 1, 1½, or 2 degrees, and even more.

To increaſe the electric tenſion, the diſcs, where ſeparated, are

This degree of electricity may appear trifling; it does not ſatisfy ſome perſons, who always wiſh to ſee effects exemplified upon a large ſcale. Be it ſo. In order to obtain electrical

cal phenomena much more diftinctly marked, I generally employ a fecond condenfer, mounted upon the electrometer itfelf, and proceed in the following manner:—I apply the plates of copper and of zinc to each other, and feparate them feveral times, at each feparation bringing one of thefe infulated plates into contact with the upper difc of the condenfer, and the other, alfo infulated, with the lower difc, which is attached to the electrometer. When this contact has been repeated ten, twelve, or twenty times, the difc of the above-mentioned condenfer being raifed, the electrometer fupports the inferior difc alone, which elevates itfelf to 10, 12, 15, 20 degrees, &c. &c.

repeatedly applied to another condenfer.

It might be imagined that, independent of the action of the condenfer, the extent of contact between the two different metals greatly contributes, as fuch, to raife the electricity to the degree which we have feen, and that we fhould obtain a much inferior degree, if they touched each other only by a few points. But I prove the contrary; that is to fay, that in the one cafe, as well as in the other, the electric tenfion rifes, during the contact, to the fame point, which is about a fixtieth part of a degree of my electrometers of thin ftraws, when the metals are zinc and copper, and a little more when they are zinc and filver; which tenfion requiring a quantity of electric fluid in the plate which performs the office of condenfer, proportionably larger, accordingly as it condenfes 60, 150, 200 times, it is evident why we obtain 1, $1\frac{1}{2}$, 2 degrees, &c. &c.

The application of the flat furfaces of the zinc and copper does not increafe their motive power.

In order to prove that the contact of two metals of little extent, and which even fubfifts only at fome points, difplaces the electric fluid in fuch a manner as to raife the tenfion in thefe metals to the fame degree; I join a fmall plate of copper with another of zinc, either fimilar or diffimilar with refpect to figure and fize, applying them to each other at a few points only, or at more points, or even foldering them together end to end. See Plate VIII. fig. 3.

Metals of other forms brought into contact by fmaller or minute furfaces.

Holding the piece *z* of zinc with two fingers, or in any other manner, I make the other, *c*, of copper, communicate with the fuperior difc of the condenfer, whilft the inferior difc communicates, as it ought, with the ground. A moment afterwards, raifing this upper difc into the air, and holding it infulated, it gives me by the electrometer from 2 to 3 degrees of

Manipulation. A fingle couple of pieces, one of which is held in the hand, and the other applied to the condenfer, produces

electricity; equal to that of the larger contacts;

of negative electricity (el. —), accordingly, as such a condenser condenses from 120 to 180 times. This proves that the electric tension of the above-mentioned plate *c* was about a sixtieth of a degree, or nearly equal to that which the two plates of copper and of zinc acquired in the preceding experiments, when applied to each other by the whole extent of their flat surfaces.

which is reversed by touching with the other metal.

When we reverse the experiment, that is to say, when we cause the plate *z* of zinc to communicate with the condenser, we likewise obtain from two to three degrees, but of positive electricity (el. +.)

If the zinc piece touch a copper condenser while the other piece (of copper) is held in the hand, no effect follows;

However, if the disc of the condenser be copper, and the plate *z* touch it immediately without any intervening substance, we obtain nothing, because the zinc being then in contact, at the two opposite ends, with copper and copper, two equal forces act in opposite directions, and by that means destroy or counterbalance each other.

unless moisture be interposed.

It is therefore necessary that the communication of the plate of zinc *z* with the copper disc of the condenser should be effected by the interposition of a conductor, which should be a simple conductor nearly, an humid conductor, such as a piece of wet card or cloth.

But *it is asserted*, that the humidity acts *merely as a conductor*, to prevent the opposite metallic contacts in the pile.

As to the rest, the action which excites and gives motion to the electric fluid does not exert itself, as has been erroneously thought, at the contact of the wet substance with the metal, where it exerts so very small an action, that it may be disregarded in comparison with that which takes place, as all my experiments prove, at the place of contact of different metals with each other. Consequently the true element of my electromotive apparatus, of the pile, of cups, and others that may be constructed according to the same principles, is the simple metallic couple, or pair, composed of two different metals, and not a moist substance applied to a metallic one, or inclosed between two different metals, as most philosophers have pretended. The humid strata employed in these complicated apparatus are applied therefore for no other purpose than to effect a mutual communication between all the metallic pairs, each to each, ranged in such a manner as to impel the electric fluid in one direction, or in order to make them communicate, so that there may be no action in a direction contrary to the others.

After

After having well afcertained the degree of electricity which I obtain from one fingle pair of thefe metallic pieces, as fhewn by the condenfer which I employ, I proceed to fhew, that with two, three, four pairs, &c. properly arranged, that is to fay, difpofed all in the fame direction, and communicating the one with the other, by as many humid ftrata (which are neceffary, in order that there may be no contrary actions, as I have already fhewn), we have exactly the double, triple, quadruple, &c. fo that if with a fingle pair we fhould be able to electrify the condenfer to that point as to make it indicate, by the electrometer, three degrees, for example, we fhould obtain fix with two, nine with three, twelve with four, &c. if not exactly, yet nearly fo. You have feen thefe experiments, and have appeared to be very well fatisfied with them, as well as Mr. Pictet, who feemed enchanted with them, and was never tired with feeing them repeated.

The intenfity of a fucceffion of couples is the fame multiple of that of a fingle couple of plates.

Here then we have already conftructed a fmall pile, which however does not yet afford any indications, by the electrometer without the aid of the condenfer. In order that it may immediately afford thefe, in order that it may arrive at one whole degree of electric tenfion, which it will fcarcely be poffible to diftinguifh, becaufe it amounts to no more than half a line on the electrometer, it is neceffary that fuch a pile fhould be compofed of about fixty of thefe combinations of copper and zinc, or, which is better, of filver and zinc, on account of the fixtieth of a degree which each combination yields, as I have already remarked. It then alfo affords fhocks if we touch its two extremities with wet fingers, and much ftronger fhocks if we touch them with metals grafped by large furfaces in the hands well moiftened, by which means a much better communication is eftablifhed.

This is the pile; and gives fhocks.

In this manner we may receive fhocks from an apparatus, whether in pile or with cups, of 30 and even of 20 pairs, provided the metals be fufficiently clean, and efpecially if the humid ftrata be moiftened, not with mere pure water, but with a confiderably ftrong faline folution.

Saline liquids, inftead of water, increafe the effect;

Thefe faline liquids, however, do not properly augment the electric force;—not at all;—they merely facilitate the paffage, and leave a freer courfe for the electric fluid, being much better conductors than fimple water, as feveral other experiments prove.

but merely, *it is afferted*, becaufe they are better conductors.

In

Experiment offered in proof. A feries of glaffes, with the metals and water, give a certain electric intenfity and fhock; but on adding falt to the water, the intenfity is the fame, though the fhock is much greater.

In order to eftablifh this fact, and render it evident to fuch as have found a difficulty in admitting it, that the electric force or tenfion is, if not entirely, yet very nearly the fame, whether the wet layers be moiftened with pure water or with a faline folution, though the difference in the fhock is fo great, I have frequently made the following experiments, of which I have fpoken to you, and which I would have fhewn to you, if I had been provided with the requifite articles. I take 30 cups or drinking glaffes, with which I conftruct one of thefe apparatufes which I term a crown of cups, putting into them a fufficient quantity of pure water, and caufing them to communicate, the firft with the fecond, the fecond with the third, and fo on fucceffively to the laft, by means of metallic wires, which terminate at one end in a plate of copper, and at the other in one of zinc, and are all turned in the fame direction. The apparatus being conftructed in this manner, I try its electric force, by caufing the firft of the cups to communicate with the ground, and applying the condenfer to a piece of metal which is partly immerfed in the laft: this condenfer, when I afterwards withdraw it, and feparate one of its difcs from the other in the proper manner, and without delay, gives me 40, 60 degrees, and more, according to its condenfing force. I alfo try the fhock in the moft advantageous manner, and find that it is very flight: after having well afcertained both the degree of the electricity and the weaknefs of the fhock, I add a pinch of falt to each cup, and repeating the proofs, I find that the electricity has not been at all increafed, the condenfer giving me ftill only 40 or 60 degrees, as before; but the fhocks are incomparably ftronger.

The apparatus charges a large battery almoft inftantaneoufly.

There are many other experiments, which I have defcribed to you verbally, and which I would willingly have performed before you, had I not been in want of the requifite apparatus. I informed you—at which you were much aftonifhed, and Mr. Pictet ftill more fo—that with one apparatus I charge a Leyden phial, whatever its capacity may be, and even a large battery; that I charge them in an inftant, or, to fpeak more accurately, in lefs than a twentieth part of a fecond, and almoft to the fame degree as the apparatus itfelf, namely, to about one degree of tenfion, if the apparatus be compofed of 60 pairs; to two degrees if it contain 120, &c.;—that I am then able to draw, by the help of the condenfer, a ftrong fpark from

from a ſmall jar charged in this manner, a great number of ſimilar ſparks from a large jar, and almoſt without limit from batteries, as I am actually able to draw them without limit of number from the apparatus itſelf.

I have informed you, that large bottles charged in this manner gave me moderate ſhocks, and batteries pretty ſtrong ones, as high as the elbow, and higher; that the ſhocks of a battery of 10 ſquare feet of coating, and charged in leſs than a twentieth part of a ſecond by one of my apparatuſes of 200 metallic pairs, are very violent and almoſt inſupportable; for I have not yet made any trial with larger batteries; but that there is every probability that the violence of the ſhocks increaſes with the ſize of the batteries, as far as a certain term, which I am not able to define; ſo that it would be poſſible, with batteries of 40, 60, and 100 ſquare feet, to give conſiderably ſtrong ſhocks, by charging them by the tranſient contact of a pile only 60, 40, 30, or ſtill fewer pairs.

Strong ſhocks from batteries ſo charged.

I have explained to you the manner in which one ought to proceed in order to perform theſe experiments with ſucceſs; that it is particularly neceſſary we ſhould carefully avoid the ſlighteſt interruptions in the communications of the conductors with the coatings of the bottles, and between each other, and that this becomes ſtill more neceſſary when the electromotive apparatus, being compoſed of a ſmall number of pairs, poſſeſſes but little power, ſo as to be unable to overcome the ſlighteſt obſtacle that might oppoſe the paſſage and the courſe of the electric fluid.

Cautions in order to ſucceed in charging a battery by galvaniſm.

Laſtly, I remarked to you, that theſe experiments confirm in a very evident manner what all the others already ſuggeſted, namely, that the quantity of electric fluid ſet in motion by my apparatus is much larger for every moment of time than that which is ſet in motion by the ordinary electrical machines; that the former afford it in much greater abundance than the latter, when the object is to produce, not an accumulation of electric fluid in inſulated bodies, in order to raiſe the electricity to a high degree of tenſion, which may be done with thoſe machines, but by no means with piles and other ſimilar apparatuſes, unleſs we alſo employ condenſers; but where we require a conſtant current of this fluid, ſupported by a continued action of a circle of condenſers not inſulated; nay, one of my apparatuſes, of only 60 or 30 metallic pairs, pours out

The pile moves a much greater quantity of electricity than any machine.

in every inftant, or in any given time, more electric fluid, if it meets with no obftacle, or if the fluid be not obftructed by the too fmall capacity of the recipient into which it is infufed, than one of the moft active electrical machines with cylinders or plates of glafs. In fact, where fhall we find a machine capable of charging a very large battery to one or even half a degree in lefs than an eighth part of a fecond, of pouring into it a fufficient quantity of electric fluid to enable us afterwards to draw from it, by the condenfer, a great number of fparks in fucceffion, as is done by one of the above-mentioned apparatufes? *

Mention of other experiments.

The other experiments, which I was able to fhew you in part, relate to the different electrofcopic phenomena which the apparatus prefents, accordingly as the one or the other of its extremities communicates with the ground, or both of them, or neither one nor the other, or as they communicate only between themfelves and with the ground at the fame time; accordingly as thefe communications are effected by perfect conductors, conductors more or lefs imperfect, &c. all which circumftances fingularly modify and produce great variations in the refults, which often appear curious and even anomalous, but which, neverthelefs, I think myfelf able to explain in a fatisfactory manner, without deviating from my principles and found electrical theory, attention being paid to the mode in which the imperfect or bad conductors act. It would carry me too far to enter at prefent into thefe details; what you have already feen, and what I have communicated, may be fufficient for the prefent occafion.

Obfervations on the preceding Memoir. W. N.

Theory of Volta too haftily adopted.

Sig. Volta, and many of our philofophical neighbours in France, will, no doubt, think, on re-confideration of the facts, that they have been too precipitate in admitting the electric energy as the only effective agent in the phenomena of the pile, and that the fluids act merely as conductors. The late interruption to correfpondence may probably have rendered the contents of my late Journal little known in France, otherwife the learned author of this memoir would

* On this fubject fee Philofoph. Journal, 4to. IV. 243—245. N.

4 have

have met a fatal objection to that part of his theory, which gives all to the metals and nothing to the fluids, in * Mr. Davy's galvanic pile, confifting of one metal throughout, but with different ftrata of fluids; for example thus; metal, cloth foaked in dilute nitrous acid, cloth foaked in water, cloth foaked in fulphuret of potafh;—then the fame metal, and nitrous acid, and water, and fulphuret;—the metal, &c. Or if a trough be ufed, the feparation between the acid and fulphuret may be made by a plate of horn, and the two fluids may be connected by a flip of wetted paper hung over the edge of the horn, which will not caufe the fluids to mix, becaufe water is lighter than either. The metals, feparately and fuccefsfully tried, were filver, copper, zinc, and lead.

Davy's galvanic pile with one kind of metal only and three fluids.

Trough of the fame conftruction.

To this I will here add, from converfation, an experiment of the fame philofopher, which is no lefs conclufive as to the direct efficacy of the fluid in this apparatus; becaufe the fame electric power is made to move either from the top or bottom of a pile of two metals, according to the nature of the interpofed fluid.—If a pile of copper and iron be conftructed as ufual, with water interpofed, the iron becomes electrified *plus* and the copper *minus*; but if the fame, or a fimilar pile, be conftructed with no other difference than that fulphuret of potafh is ufed inftead of water, the iron is electrified *minus* and the copper *plus*. In the firft cafe the iron is oxided; but in the fecond there is no oxidation of this metal, and the copper is oxided, and probably fulphurated.

Electric current of the common pile reverfed by changing the fluid.

Laftly, we have another inftance of the power and importance of the fluids in the article which follows the prefent, where charcoal alone is ufed, and the leading condition is, that two different fluids fhall be ufed.

Galvanic feries of charcoal only and two fluids.

As we know by the experiments of De Sauffure, and many others, that chemical changes do difturb the equilibrium of electricity, and they certainly take place in the pile, it feems at leaft probable that thefe may have the chief agency in the apparatus. With regard to the principle of electric motors of Sig. Volta, I muft obferve, that Bennet made many direct experiments of applying different metals, by the fingle

Chemical changes are known to produce electricity; and as fuch changes happen in the pile, they may probably caufe its electricity.

* See notice in the Scientific News, Phil. Journal, 4to. for May, 1801, Vol. V. p. 78; and the fubject is fully treated in the Philof. Tranf. for 1801, in a paper inferted in the fame volume of our Journal, 4to. p. 341.

and

Bennet made experiments on electricity by contact of metals before A. D. 1789.—Cavallo before 1795.

and double touch, to the plates of the doubler, followed by the production of electricity, by him called adhesive electricity, which were published in his *New Experiments on Electricity* in 1789, pages 86 to 102; and others were made by Cavallo o the electricity produced by the contact or stroke of a piece o metal, let fall out of the hand, for the most part, upon an insulated metallic plate, which were published in the third volume of his *Electricity* in 1795. I do not know the date of Volta's experiments, but believe them to be much later than

They supposed various electric capacities; but the hypothesis of a current of electricity produced by metallic contacts belongs to Volta.

those of the same kind by Bennet. This last philosopher, as well as Cavallo, appears to think that different bodies have different attractions or capacities for electricity; but the singular hypothesis of electromotion, or a perpetual current of electricity being produced, by the contact of two different metals is, I apprehend, peculiar to Volta.

XII.

An Account of a Method of Constructing Simple and Compound Galvanic Combinations, without the Use of Metallic Substances, by means of Charcoal and different Fluids. By Mr. DAVY.

Charcoal between nitric acid and water affords galvanic effect.

1. IF a piece of well burned charcoal be brought in contact at one of its surfaces with a portion of water, and at another surface with a portion of nitric acid, a simple galvanic combination will be formed when the two fluids are connected together. And the powers of it are demonstrated by its agency upon the limbs of frogs, and by its effects upon the organs of sense.

Series of such combinations.

2. A compound galvanic combination or a galvanic battery may be formed from a number of series composed of the same substances: but in this case the fluid elements of each series, not being immediately in contact, must be connected with similar elements in other series in an order of regular alternation, such as water, charcoal, acid; water, charcoal, acid; and so on.

Charcoal battery by a series of glasses alternately charged with nitric acid and water, and al-

3. The best mode that has yet occurred of constructing galvanic batteries with charcoal, is by means of a number of glasses, which are made to contain, alternately, nitrous acid, and water, and which are connected in pairs by means of moistened

3

tened cloth. The pieces of charcoal used are made from very denfe wood, fuch as box, or lignum vitæ; and in this cafe the fluids will not penetrate into them by capillary attraction, much beyond the places of their primary contact. Their forms are thofe of arcs, fo that each piece connects together two glaffes; but in inftances where fingle pieces of charcoal cannot be obtained of the proper fhape, two long and thin flips may be faftened together by filk, fo as to form the angle neceffary to their infertion into the glaffes.

ternately connected by charcoal and flips of wet cloth.

4. Twenty feries in a battery of this kind produce fenfible but feeble fhocks, and when a fingle metallic feries with a gold wire and two glaffes of water is fubftituted for one of the primary feries, hidrogen is given out by the metallic point in the glafs of water in the place of the acid, whilft oxigen is evolved from the point in the other glafs.

Its power.

5. In the galvanic batteries with charcoal, fulphuric acid may be fubftituted for nitric acid; and folution of fulphuret of potafh for the water, without any material alteration in the nature of the agency; the folution of the fulphuret indeed, feems, in fome meafure, to increafe its intenfity, and combinations containing this fubftance, denfe charcoal, and concentrated nitric acid, appear to be fuperior in activity to fimilar combinations containing copper and the fame fluid elements, and to be nearly equal to thofe compofed of zinc, filver, and water.

Other fluids.

January 9, 1802.

XIII.

Short Analyfis of the Principles and Structure of Mr. CLOSE's *Hydraulic Engine.*—W. N.

AN eminent engineer, to whofe talents and invention fociety is highly indebted, writes to me that he cannot underftand Mr. Clofe's engines, and requefts fome explanation, as he does not conceive that impoffibilities would have my fanction. Though I am convinced that an attentive revifal of the defcriptions would render my elucidation unneceffary, yet as that which he requefts may not be unacceptable to other readers, I have here annexed a few obfervations; viz.

An explanation of Mr. *Clofe's* engines requefted.

Short enumeration of its principles and mode of operation.

1. That the water (of which part is intended to be raifed above the level) muft have a fall. 2. That this fall affords the ufual difference between the legs of the fyphon. 3. That if the fyphon had two or more afcending legs, water would rife through them all. 4. That if one of thefe legs were fmall and contained air, that air would pafs into the great fyphon, and if the velocity of the fluid were rapid enough, the bubbles would pafs down along with the water, inftead of rifing to the top and breaking the column. 5. That if there were a contrivance of two cocks in the water way of the fmall afcending leg, and both were fhut, the portion of water might be drawn out from between them by a fide cock, and that fpace filled with air. 6. That this fpace may, if thought fit, be a globe or other veffel. 7. That this fpace being above the level, the water thus drawn off *is in fact raifed.* 8. That by fhutting the fide cock, and opening the two others, the included air would afcend and be carried down the great fyphon. 9. After which another like portion of water might be drawn out as before, &c. &c. And, 10. It is clear that by fmall fyphon work the cocks might be opened and fhut without attendance.

The water is raifed as ufual by its fall.

This is only one of the many methods of raifing water above its level by virtue of its fall. It certainly poffeffes merit and originality of invention; but whether it be generally preferable to other known methods is another queftion. For my part, I think it is of advantage to our general ftock of knowledge and invention, to publifh all ingenious novelties, without any particular folicitude refpecting the extent of their power. A variety of circumftances and local fituations afford opportunities of applying engines, or their parts, with fingular advantage, which in the general contemplation of their ftructure and ufe, might feem to be more ftriking for their fingularity than their effect.

Utility of publifhing new inventions.

XIV. *Simple*

XIV.

Simple Process for taking a Copy of a recent Manuscript. Communicated to the Philomathic Society at Paris by Charles Coquebert.

THIS process is the more interesting as it requires neither machine nor preparation; and may be used in any situation. It consists in putting a little sugar in common writing ink, and with this the writing is made on common paper sized as usual; when a copy is required; this unsized paper is taken and lightly moistened with a sponge. The wet paper is then applied to the writing, and a flat iron, such as is used by laundresses, of a moderate heat, being lightly passed over the unsized paper, the counter-proof or copy is immediately produced *.

Sugar is put in the ink. Damp unsized paper is laid on the writing. The impression or copy is made by an heated iron.

XV.

Narrative and Explanation of the Appearance of Phantoms and other Figures in the Exhibition of the Phantasmagoria. With Remarks on the Philosophical Use of common Occurrences.— W. N.

A VERY striking application of the magic lanthorn has been made this winter to the public amusement by M. Philipsthal at the Lyceum. The novelty consists in placing the lanthorn on the opposite side of the screen which receives the images, instead of on the same side as the spectator, and suffering no light to appear but what passes through, and tends to form those images. His sliders are therefore perfectly opake, except that portion upon which the transparent figures are drawn, and the exhibition is thus conducted.

Phantasmagoria. New application of the magic lanthorn by placing it on the opposite side of the screen, and making the figures alone transparent.

* I have not heard who was the original inventor of this useful process. That sugar prevents ink from speedily drying has been long known, and this method of impression has been used in some of our public offices, and elsewhere in England for some years.

W. N.

All

Account of the exhibition of Mr. Philipsthal.

All the lights of the ſmall theatre of exhibition were removed, except one hanging lamp, which could be drawn up ſo that its flame ſhould be perfectly enveloped in a cylindrical chimney, or opake ſhade. In this gloomy and wavering light the curtain was drawn up, and preſented to the ſpectator a cave or place exhibiting ſkeletons, and other figures of terror, in relief, and painted on the ſides or walls. After a ſhort interval the lamp was drawn up, and the audience were in total darkneſs, ſucceeded by thunder and lightning; which laſt appearance was formed by the magic lanthorn upon a thin cloth or ſcreen, let down after the diſappearance of the light, and conſequently unknown to moſt of the ſpectators.

Darkneſs. Thunder. Lightning.

Figures of the departed; phantoms, &c.

Theſe appearances were followed by figures of departed men, ghoſts, ſkeletons, tranſmutations, &c. produced on the ſcreen by the magic lanthorn on the other ſide, and moving their eyes, mouth, &c. by the well known contrivance of two or more ſliders. The transformations are effected by moving the adjuſting tube of the lanthorn out of focus, and changing the ſlider during the moment of the confuſed appearance.

The figures ſeem to recede to an immenſe diſtance.

It muſt be again remarked, that theſe figures appear without any ſurrounding circle of illumination, and that the ſpectators, having no previous view or knowledge of the ſcreen, nor any viſible object of compariſon, are each left to imagine the diſtance according to their reſpective fancy. After a very ſhort time of exhibiting the firſt figure, it was ſeen to contract gradually in all its dimenſions, until it became extremely ſmall and then vaniſhed. This effect, as may eaſily be imagined, is produced by bringing the lanthorn nearer and nearer the ſcreen, taking care at the ſame time to preſerve the diſtinctneſs, and at laſt cloſing the aperture altogether: and the proceſs being (except as to brightneſs) exactly the ſame as happens when viſible objects become more remote, the mind is irreſiſtably led to conſider the figures as if they were receding to an immenſe diſtance.

How effected.

Transformations.

Several figures of celebrated men were thus exhibited with ſome transformations; ſuch as the head of Dr. Franklin being converted into a ſkull; and theſe were ſucceeded by phantoms, ſkeletons, and various terrific figures, which inſtead of ſeeming to recede and then vaniſh, were (by enlargement) made ſuddenly to advance; to the ſurprize and aſtoniſhment of the audience, and then diſappear by ſeeming to ſink into the ground.

Skeletons, &c. ſuddenly ruſh forward.

This

This part of the exhibition, which by the agitation of the spectators appeared to be much the most impressive, had less effect with me than the receding of the figures; doubtless because it was more easy for me to imagine the screen to be withdrawn than brought forward. But among the young people who were with me the judgments were various. Some thought they could have touched the figures, others had a different notion of their distance, and a few apprehended that they had not advanced beyond the first row of the audience.

The deceptions not equally effectual to all the spectators.

As I have given this account, of an exhibition on which an ingenious mechanic in part depends for his support, it will not be impertinent to my present and future readers to add, that the whole, as well as certain mechanical inventions, were managed with dexterity and address, and that his gains in London have been very considerable. The figures for the most part are but poorly drawn, and the attempt to explain the rational object, or purpose of the exhibition was certainly well intended; but unfortunately for the audience his English was unintelligible. His lightning too, being produced by the camera was tame, and had not the brisk transient appearance of the lightning at the theatres, which is produced by rozin, or lycopodium powder, thrown through a light, which in Mr. P's utter darkness might easily have been concealed in a kind of dark lanthorn.

Observations on the performance.

Lightning.

My young pupils on their return made drawings, and applied the magic lanthorn to a sheet in a door way between two rooms. Some of their drawings were made on thin paper and varnished, to render them transparent, and others were on glass. The paper figures were less bright than the others; but an advantage may be had in this material by those who cannot draw, because they may colour and varnish small figures, engraved in aqua-tinta or in any other manner without stroke.

Imitation by some young persons.

A plate of thin sheet iron, such as German stoves are made of, is an excellent instrument for producing the noise of thunder. It may be three or four feet long, and the usual width. When this plate is held between the finger and thumb by one corner, and suffered to hang at liberty, if the hand be then moved or shaken horizontally, so as to agitate the corner at right angles to the surface, a great variety of sounds will be produced; from the low rumbling swell of distant thunder, to the succession of loud explosive bursts of thunder from elevated

Remarkable imitation of thunder by a sheet of iron.

clouds,

clouds. This ſimple inſtrument is very manageable, ſo that the operator ſoon feels his power of producing whatever character of ſound he may deſire; and notwithſtanding this deſcription may ſeem extravagant, whoever tries it for the firſt time wlll be ſurprized at the reſemblance. If the plate be too ſmall, the ſound will be ſhort, acute, and metallic.

To Correſpondents.

The preſent ſeems to be no unfit place to notice ſome correſpondents which my late paper on ſhaving has induced to favour me with a few letters. To thoſe who find themſelves inſtructed and gratified by a ſmall addition to their daily comforts, I can only give my congratulation; but to that friend who calls himſelf *a Shaver*, but who I fear is no true ſhaver, from the little veneration he ſeems to have for the art, I am to return my thanks for his *Eſſay on the Art of cutting Bread and Butter*, and muſt ſay, that if it had abounded with inſtruction equal to its merriment, I ſhould have been glad to have given it to my readers. He who ſhall invent a machine to perform even this operation, will, I think, deſerve well of ſociety. I would humbly propoſe it as my opinion, not without expectation that many others may think with me, that no ſubject is beneath the conſideration of a philoſopher. Our beſt philoſophers have been moſt ſtudious of the daily occurrences of life. Newton's attention was attracted by the fall of an apple before he extended the theory of gravitation to the moon. Soap bubbles and the priſm were play-things before he ſelected them as inſtruments to analyſe the rays of light. Franklin by the kite of a child conducted lightning from the clouds to the earth; and in a word, it appears that the greateſt diſcoveries have been made, not by thoſe who could command the expenſive and ornamental apparatus of ſhowy experiment, but by ſuch as were in the habit of cloſe attention to the means, the agents, and the operations which are conſtantly performed around us, and frequently ill underſtood becauſe habitually neglected.

Ludicrous Eſſay.

Our beſt philoſophers have profited moſt from common occurrences.

But laughter is ſaid to be one of the diſtinctive characters of the human ſpecies, and our pleaſures are not ſo many that we ſhould reject any innocent ſource of amuſement. No ſerious evil is to be apprehended from raillery, eſpecially if at-

Ridicule not injurious to the progreſs of philoſophy.

4 tended

tended with good nature. Herfchel will not be diverted from purfuing his difcoveries, becaufe the inhabitants of Laputa give themfelves too much anxiety about the fun's good health. Let the man of wit enjoy his joke, and the man of experiment his rational toys, provided both unite in cultivating that amiable fpirit of philanthropy, on which the happinefs of focial intercourfe fo much depends.

SCIENTIFIC NEWS.

Extract of a Letter from Dr. Lorenz de Crell. *(Helmftadt, Dec. 14, 1801.) On the Chlorophane and a fuppofed Variety of Barytes.*

THE Prince of Gallitzin, F.R.S. poffeffor of a moft excellent mineralogical cabinet, and living now as a private individual at Brunfwick, has obferved that the chlorophane, or that Siberian fluſs-fpar, which, after being gently heated on charcoal, turns green (on account of which it is called pyrofmaragdus in the Chemifche Annalen) and on being cooled regains its former colour; that the chlorophane, I fay, loft this property on being expofed in a tin box to the rays of the fun in a window during the fummer. It was turned quite colourlefs, and exhibited no light at all on being heated.— The Count of Hoffmanfegg, whofe travels to Portugal (for difcoveries in natural hiftory) were written by Profeffor Linck, his companion, has brought with him two ftones, greatly refembling quartz, or rock cryftal externally, but without regular figure; giving fparks with fteel, but of far greater fpecific gravity, viz. 3,500. It fhewed fome electricity on friction. This new kind of ftone (for fuch it is thought by good mineralogifts) is to be analyfed chemically. In the mean time Dr. Bruckman, who is well known by his books on precious ftones, thinks proper to call it *barytes nobilis*.

The chlorophane lofes its luminous property by expofure to light.

Barytes nobilis.

Corrindon or Adamantine Spar found near Philadelphia.

M. Guillemard, a learned mineralogift, who has travelled much in North America, writes to Dr. Delamétherie, that Mr. Adam Seybert has difcovered corrindon or adamantine fpar

Corrindon or adamantine fpar found in America.

ſpar at Cheſnut-hill, about ten miles from Philadelphia. It is known that the ſea coaſt in theſe parts is entirely granitic, and this granite extends to a conſiderable diſtance inland. Farther inland ſucceeds the gneiſs; then micaceous ſchiſtus; then grit ſtone; and, laſtly, the ground towards the lakes Ontario and Erie is all calcareous.

The granites of the coaſt of Philadelphia contain various marked veins of granite, as is obſerved in different granitic ſoils.

It was in one of theſe granitic veins, which may be almoſt compared to loads (filons), that Mr. Seybert found corrindon mixed with the other ſubſtances or elements of granite which compoſe the vein.

J. de Phyſ. Brum. 10.

Nature of the Earth which is eaten by the Inhabitants of New Caledonia, aſcertained by CIT. VAUQUELIN.

Savages on the Oronoko eat earth, which is thought to be nouriſhing.

Humboldt, in a letter to Fourcroy, of which an extract was given in the 50th number of the Bulletin des Sciences, affirms, that the Otomagues almoſt entirely ſubſiſt on a kind of earth for three months, when the Oronoko is ſo high that they can find no more turtle. Some of them eat a pound and a half daily. It is affirmed by the miſſionaries that they mix it with the fat of the crocodile's tail; but Mr. H. inſiſts that they do nothing but ſlightly burn and moiſten it. He thinks that moiſtened earth may be nouriſhing by decompoſing the air, or by ſome effect of the chemical affinities.

Inhabitants of New Caledonia alſo eat earth.

Cit. Labillardiere has ſtated a fact equally ſingular, from an obſervation made in a part of the world conſiderably diſtant from that inhabited by the Otamagues; that the inhabitants of New Caledonia, when preſſed by hunger, eat a conſiderably large quantity of a greeniſh ſteatites, which is ſoft and friable. It may be eaſily imagined how the dreadful cuſtom of eating priſoners of war might have been introduced amongſt a ſavage people, who are reduced to ſuch want as to be obliged to ſatisfy their hunger by diſtending their ſtomach and inteſtines with an earthy ſubſtance, which has no other alimentary quality but that of being light and friable.

Cit.

Cit. Vauquelin, wiſhing to know the nature of this earth, and to diſcover if it contained any nutritive parts, has analyſed it by the uſual methods, from ſome ſpecimens which were ſent him by Cit. Labillardiere. This, by analyſis, found to contain no nutriment.

This earth is ſoft to touch, compoſed of ſmall fibres, which are eaſily ſeparated, and when ignited it loſes $\frac{4}{100}$ of its weight. It is compoſed of,

37 parts of pure magneſia.
36 of ſilex.
17 of oxide.
3 or 4 of water.
2 or 3 of lime and copper.

It does not therefore contain any alimentary parts, and can only be conſidered as filling the ſtomach; a kind of mechanical expedient to ſuſpend the pains of hunger.

Soc. Philomath. No. 55, An. 10.

Account of a blue Oxide of Iron. By CIT. VAUQUELIN.

This ſubſtance was ſent to the Counſel of Mines, by the Baron de Molt. It is of a light blue colour, and has the form of ſmall inſulated maſſes in the cavities or clefts of quartz and hard green ſteatites. It is friable, but rather greaſy to touch, and changes its colour before the blow-pipe, and at laſt melts into a greeniſh white glaſs. Blue oxide of iron not yet analyſed.

Neither acids or weak alkalis alter its colour. Theſe circumſtances diſtinguiſh it from lapis lazuli and the pruſſiate of iron.

This ſubſtance communicates a ſaffron yellow to the muriatic acid in which it was digeſted, and changes its own colour a little, but cannot be diſcoloured unleſs it be at the ſame time diſſolved. There only remains a ſmall quantity of ſilex, which appears to have conſtituted its gangue.

On examining the muriatic acid that was uſed for this operation, it was found it had taken up alumine, lime, and oxide of iron, but neither ſulphurated hidrogen, mangaueſe, nor phoſphoric acid were diſcovered in it, ſubſtances to which the blue colour of this oxide of iron might be aſcribed. It therefore remains to be determined what is the cauſe of the remarkable colour of this oxide, a colour which has never yet been

been given to this metal by any chemical procefs. It appears only that the iron in this oxide is carried to a degree of oxigenation nearly approaching to the *maximum.*

Bulletin de Sc. No. 55, An. 10.

Ufe of Whifkers in certain Quadrupeds. By M. VROLYK, *Profeffor of Natural Hiftory at Amfterdam.*

The whifkers of animals appear to be ufeful to feel their way in irregular cavities.

This naturalift has endeavoured to afcertain, by experiment, of what ufe the long ftiff hairs, termed *whifkers,* which are placed near the mouth of certain quadrupeds, might be to them. Having difpofed a number of books, placed on their edges, upon the ground in his chamber, fo as to form a kind of labyrinth, he let loofe a rabbit, with his eyes bound up, amongft them. The animal fucceeded in extricating himfelf from this labyrinth, without having overturned them; but when Mr. Vrolyk had cut off his whifkers, the animal, having no longer thefe tentacula to direct him, ran againft the books, and overturned them. Moreover it is known that the bulb, in which each of the whifkers is implanted, receives a fmall nervous filamen proceeding from the infra-orbilaty nerve.

Soc. Philom. No. 50, X.

Galvanic Charge of a large Battery by the Metallic Pile. By Dr. VAN MARUM.

The Harlem battery charged by galvanifm.

Dr. Van Marum, of Harlem, has charged 25 large electric jars (containing 137½ fquare feet of coating) by an almoft inftantaneous contact of a galvanic pile of 200 pair of 3 Guilder filver pieces, alternated with zinc and moift cloth. The electric intenfity was equal to that of the pile itfelf, but the fhock much lefs. The fhock of the battery was generally equal to that of half the number of pairs employed in its charge. It was, however, very ftrong, and reached up to the fhoulders. As the 120th Number of the Annales de Chemie, in which his letter is inferted, arrived late in the month, I muft defer the reft of the account.

ACCOUNT

ACCOUNT OF BOOKS OF SCIENCE.

A Syllabus of a Course of Lectures on Chemistry, delivered at the Royal Institution of Great Britain. By Mr. Davy. 90 pages, 8vo. Cadell, London.—On Thursdays and Saturdays at two, and on Thursdays at eight o'clock, P.M. The morning lectures are read on general chemistry, and the evening lectures on the connection of chemistry with the arts.

Davy's chemical lectures.

The present course is divided into three parts. Part I. The chemistry of ponderable substances; under six divisions. 1. Of the chemical powers, and the modes of their application. 2. Of undecompounded substances, or simple principles. 3. Of bodies composed of two simple substances. 4. Of bodies composed of more than two simple substances. 5. Of substances composed of different compound bodies, or of compound bodies and simple bodies. 6. Of the general phenomena of chemical action. Part II. The chemistry of imponderable substances; under four divisions. 1. Of heat or caloric. 2. Of light. 3. Of the electrical influence. 4. Of galvanism. Part III. The chemistry of the arts; under eight divisions. 1. Of agriculture. 2. Of tanning. 3. Of bleaching. 4. Of dyeing. 5. Of metallurgy. 6. Of the manufactory of glass and porcelain. 7. Of the preparation of food and drink. 8. Of the management of heat and light artificially produced.

It is unnecessary to make any general remarks on the benefits derived to science from correct and sufficiently extensive outlines of facts and doctrines published from time to time by their active cultivators. The present work must prove eminently useful to the pupils of the lecturer, and will add to his reputation by its perspicuity and order. In the rapid progress of experimental discovery, even those learned men who attend to the general state of natural philosophy, without closely following its minuter variations, will be essentially served by clear notices of the present state of one of its chief departments.

The

The Elements of Book-keeping: comprising a System of Merchants' Accounts, founded on real Business, and arranged according to Modern Practice. By P. Kelly, *Master of Finsbury Square Academy.* Price 5s. Johnson, Robinsons and Rivingtons.

Kelly's Elements of Book-keeping.

The improvements which time and experience have effected of book-keeping, do not comprehend any change in the original principle of double entry; but in the arrangement and classification of similar accounts, which facilitate the operations of commerce nearly in the same manner as the business of manufactures is expedited by the division of labour. The elementary treatise before us exhibits these improvements.

The author introduces his work by a concise account of the history of this art, which he thus concludes: "from the foregoing view of the principal authors who have written on book-keeping, it appears that they have been composed of two different descriptions, possessing very distinct qualifications. The first, and by far the most numerous class, were teachers who have explained the principles, without adverting to the progressive improvements of practice; and the second, merchants who have exhibited those improvements, without explaining the principles. The productions of both classes of writers are highly useful, and to combine their utility is the object of the present undertaking."

This work comprises three sets of books, the first explains and illustrates the principles of both single and double entry; the second is a further exemplification of the Italian method of double entry by waste book, journal, and ledger, which does not differ from the systems given by other authors; but the third set of books differ essentially, and therefore deserves particular notice. Here the waste-book is divided into a number of subsidiary books, each of which is the register of its peculiar portion or department of business, and each book is divided into monthly transactions. By this means the journal is greatly shortened and simplified; but t principal advantage of this systematic arrangement consists in posting to the ledger; for there a whole month's cash, bills, commission, insurance, or interest, are each carried in one sum or entry from the journal to the ledger. This method, by which repetitions are avoided, and labour considerably diminished, is now generally adopted in many principal mercantile houses. The author

informs

informs us in his preface, that "he has had accefs, for many years, to the books of feveral eminent merchants; and that he has endeavoured in all cafes to follow the moft approved precedents of mercantile experience."

In the third fet of books the invoices, fales, and other tranfactions, have been felected from merchants books:—the advantage of founding a fyftem of book-keeping on real occurrences are obvious; for although the principle of double entry can be explained by fictitious examples, yet thefe may give the learner wrong notions of bufinefs; whereas from real tranfactions, he will obtain fo much practical information of commercial affairs in general, as muft intereft the mind, and more effectually fix the attention in the courfe of ftudy.

Memoirs fur l'Influence de l'Air, &c.—Memoirs concerning the Influence of the Air, and feveral gafeous Subftances, on the Germination of various Kinds of Grain. By Huber and Senebier, 1 Vol. in 8vo. Geneva. Pafchoud, 1801.

Huber and Senebier on the influence of air on vegetation.

This work is curious from the circumftances under which it was compofed. Cit. Huber, already well known for his writings on Bees, is blind, notwithftanding which he has performed thofe experiments which were fuggefted to him by C. Senebier. The experiments were made in order to determine the influence of various gafes, particularly the oxigen gas on germination *. The feeds were placed either on wet flannels or fponges under receivers filled with gas; the principal refults were as follows. All the grains placed in the azotic gas remained unaltered, but germinated on being afterwards placed in the open air. Their growth was accelerated, though flightly, in pure oxigen, but was more vigorous in that which contained a little carbonic acid. In this experiment the carbon of the grain combines with the oxigen, and forms carbonic acid gas. The feeds fprung up in artificial atmofpheric air, the fame as they would have done in common air. The proportions moft favourable for germination, are three meafures of azote or hidrogen to one of oxigen. The grains which did not grow when included

* On this fubject fee alfo Cruickfhank, in the Phil. Journal, 4to, Vol. I. page 339, for Nov. 1797.

 in

Huber and Senebier on the influence of air in vegetation.

in azote, were not affected by a proper quantity of oxigen being introduced by degrees, but shot forth very well when the same quantity was introduced at once. This difference may be explained by considering, that in the first case the oxigen is successively employed in receiving from the grain the carbon which is disengaged, so that none remains to vivify it, but when it is poured in at once, there is sufficient for both purposes.

Seeds do not germinate either in carbonic gas, or pure hidrogen gas. One lettuce seed absorbs in order to germinate a portion of oxigen, which is equal to 26 milligrammes of water, (half a grain) and it does not grow unless the oxigen is at least the eighth part of the atmosphere in which it vegetates. A great quantity of carbonic acid gas is more injurious to germination than the azote, and the azotic gas more so than hidrogen.

If seeds be put to grow in hidrogen gas, the carbon of the grains dissolves in it, and combines very intimately.

The vapour of sulphuric ether placed under a recipient of atmospheric air, prevented the seeds from growing, without altering the quantity of oxigen in the air. The same happens with camphor, oil of turpentine, assa-fœtida, vinegar and ammonia. Putrefied bodies prevent germination, by the abundance of car-bonic acid gas they emit. It appears from the preceding facts that oxigen is indispensibly necessary to germination, and that it serves to carry off from the grain that carbon which is disengaged by fermentation. But this rule is not without its exceptions.

In fact peas have germinated in water deprived of air by every possible means, and at whatever depth they have been immersed. The seeds of beans, lentilles, spinach, lettuce and corn, grow in the same manner beneath the water with greater or less facility. These seeds germinate better in water charged with oxigen gas, than in water entirely deprived of it, but not at all in water charged with carbonic acid. The acids retard their germination. The air emitted by peas under pure water, is a mixture of carbonic acid and carbonated hidrogen.

Peas have germinated in pure hidrogen gas and in air, where other seeds have already germinated, and they have totally exhausted the oxigen it contained. In this experiment the hidrogen gas becomes loaded with carbon. They likewise

wiſe germinated in azote gas. They do not germinate in oil; but if after having been ſwelled in water they are put into oil, they germinate very well.

Theſe facts afford new inductions in favour of the decompoſition of water in germination, and conſequently in vegetation.

Bull. des Sc. x. No. 55.

Mémoires d'Agriculture, &c.—p. 79.

Memoirs of the Agricultural Society of the department of the Seine.

At the head of this volume are the order of the prefect for printing it, the rules of the Society, and a liſt of its ordinary and correſponding members, with an account of the Society's labours: after which are the following memoirs.—Inſtructions for preſerving wheat from ſmut: by CC. Cadet-de-Vaux, Parmentier, Saint-Genis, and Yvart.—Report of the experiments of C. Houdart, junior, on preparing and economiſing ſeed.—On the means of draining various lands by ſimple and not expenſive proceſſes, by C. Chaſſiron. This memoir, which has been publiſhed ſeparately, is preceded by ſome obſervations on the general ſyſtem of inland navigation in France, and followed by a comparative table of the coal-mines worked in each department, and of thoſe which require only navigable canals and rivers to enable them to be worked. The former are fifty-one in number; the latter, ſixty. Their importance to thoſe of the French manufactures that require fuel, or mechanic arts, and agriculture, muſt be obvious, at a time when they are perhaps not ſufficiently aware of the want of wood, with which they are threatened, from the great devaſtation of foreſts in moſt parts of the republic; a ſubject which has been treated of by C. Laſterie in a ſeparate memoir in this volume. —On the preciſe ſignification of the terms *agriculture* and *rural economy*, by C. Cels.—Some reflections on the ſuppoſed number of ſheep in France, by C. Delong. The intention of this memoir is to excite inquiry concerning this important ſubject.—The advantages of nurſeries on eſtates of a certain extent, in facilitating ſuch annual plantations as may be ſuited to them, by C. Villele.—An intereſting account of the ſucceſsful amputation of the fore-leg of a cow, which had been fractured, by C. Chaumontel.—Experiments, inquiries, and obſervations on elms, by C. Boucher; to which C. Denorgelles has added his experiments for obtaining alcohol from the ſap of vegetables.—On the product of different ſorts of wheat that are cultivated, and the melioration of them; or the advantages that would reſult from inquiring what ſorts of

wheat

wheat yield the moſt bread; by C. Chancey.—On rural edifices, by C. Garnier-Deſchénes.—On the manner in which the mountains in the Cevennes are fertilized, by C. Chaptal.—On the means of rendering fit for uſe wells that have been abandoned in conſequence of the mephitization of the ſoil, by C. Cadet-de-Vaux.—Obſervations on the working of mines, by C. Creuſé-Latouche.—On the cultivation of the ſugar-cane in the Caribbee iſlands, and particularly of that of Otaheite by C. Moreau-St.-Mery.—On the cultivation of the ſugar-cane, by C. Coſſigny.—On the cinnamon-tree of French Guiana, by C. le Blond.—The volume concludes with accounts of the lives of Cretté de Pallnel, and T. Francis de Grace.

BOOKS OF SCIENCE,

Imported by Deboſſe, of Gerrard-Street, Soho.

Books of ſcience imported.

HISTOIRE Naturelle des Oiſeaux de Paradis, par Viellot, No. 13, 1l. 15s,

Dictionnaire raiſonné & univerſel des Arts & Métiers, par Jaubert, 1801. 5 Vol. 8vo. 1l. 11s. 6d.

Manuel du Voyageur à Paris, An. X. 2s. 6d.

Voyage pittoreſque & phyſico-economique dans le Jurat, par Lequinio, An. ix. 2 Vol. 12s.

Geographie, moderne & univerſelle, par Lacroix, précédé d'une Traite de la Sphere & d'un d'Aſtronomie, 1800, 2 Vol. 8vo. avec Cartes, 18s.

Nouveau Dictionnaire François-Italien, & Italien-François, Abregé d'Alberti, 1801. 2 Vol. 8s.

Reſpecting agricultural information.

In anſwer to the obliging letter of *A friend to Agriculture*, recommending that ſubject to conſtitute part of the preſent Journal, I am to obſerve, that though this moſt intereſting department of human induſtry and reſearch is of the firſt importance to ſociety, and its proceſſes are ſcientific throughout, yet from the complicated number of cauſes to be developed, the conſiderable time required for experiment, the large ſcale of operation, the uncertainty of reporters, and other conſiderations, I have been leſs forward to preſs this object, than from its high value I ſhould elſe have been. Communications, ſelections, or accounts, which from their authenticity and their deciſive effect, may tend to eſtabliſh points of eſſential value, will undoubtedly conduce to the purpoſes of our publication, and in this reſpect I ſhall thankfully avail myſelf of the liberal offer of my correſpondent.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1802.

ARTICLE I.

Description of an Engine which operates by the Pressure and Descent of a Column of Water against a Piston; nearly in the same Manner as the double Steam Engine operates by Means of Steam. Communicated by the Inventor Mr. Richard Trevithack, *of Camborne, near Truro, in Cornwall.*

Application of an inclosed column of water as a first mover.

In a late conversation with the inventor of the engine which forms the subject of the present memoir, the method of applying a column of water as a first mover, by its alternate action upon a piston in a cylinder, was mentioned by me as likely to prove advantageous in a variety of situations, and probably to afford as beneficial a result with regard to power, as any of the other ways of applying a fall of that fluid. It was with no small gratification, that I found this skilful engineer had amply considered the subject, and had carried it into full effect. I immediately saw, that the communication of results so interesting and authentic, would be highly acceptable to my readers, and honourable to the Journal. Upon my request, he with great readiness and liberality supplied me with the drawing exhibited in Plate IX. and had the goodness to dictate the materials, whence the following description was made.

 The

Hiftory of a new engine for raifing water by an inclofed column.

The engine, of which a fection is here given on a fcale of half an inch to a foot, was erected three years ago at the Druid Copper Mine, in the parifh of Illogan, near Truro, in Cornwall.

Defcription of the parts, and their organization.

It was worked about two years; but the mine has not proved fufficiently beneficial to encourage the proprietors to go on during the laft twelvemonth. A B reprefents a pipe fix inches in diameter, through which water defcends from the head to the place of its delivery to run off by an adit at S, through a fall of 34 fathom in the whole; that is to fay, in a clofe pipe down the flope of a hill 200 fathoms long, with 26 fathoms fall; then perpendicularly fix fathoms, till it arrives at B, and thence through the engine from B to S two fathoms. At the turn B the water enters into a chamber C, the lower part of which terminates in two brafs cylinders four inches in diameter; in which two plugs, or piftons of lead D and E, are capable of moving up and down by their pifton rods, which pafs through a clofe packing above, and are attached to the extremities of a chain leading over, and properly attached to the wheel Q, fo that it cannot flip.

The leaden pieces D and E are caft in their places, and have no packing whatever. They move very eafily; and if at any time they fhould become loofe, they may be fpread out by a few blows with a proper inftrument, without taking them out of their place. On the fides of the two brafs cylinders, in which D and E move, there are fquare holes communicating towards F and G, which is an horizontal trunk or fquare pipe, four inches wide and three inches deep. All the other pipes G, G and R are fix inches in diameter, except the principal cylinder wherein the pifton H moves; and this cylinder is ten inches in diameter, and admits a nine foot ftroke, though to accommodate the drawing to our Journal, it is here delineated as if the ftroke were only three foot.

The pifton rod works through a ftuffing box above, and is attached to M N, which is the pit-rod, or a perpendicular piece divided into two, fo as to allow its alternate motion up and down and leave a fpace between, without touching the fixed apparatus or great cylinder. The pit-rod is prolonged down into the mine, where it is employed to work the pumps, or if the engine were applied to mill work, or any other ufe, this rod would form the communication of the firft mover.

K L,

New engine for raifing water by the defcent of a column inclofed in a pipe.

K L, is a tumbler or tumbling bob, capable of being moved on the gudgeons V, from its prefent pofition to another, in which the weight L fhall hang over with the fame inclination on the oppofite fide of the perpendicular, and confequently the end K will then be as much elevated as it is now depreffed.

The pipe R S has its lower end immerfed in a ciftern, by which means it delivers its water without the poffibility of the external air introducing itfelf; fo that it conftitutes a torricellian column, or water barometer, and renders the whole column from A to S effectual, as we fhall fee in our view of the operation.

Operation, or working procefs.

The operation. Let us fuppofe the lower bar K X of the tumbler to be horizontal, and the rod F O fo fituated, as that the plugs or leaden piftons D and E fhall lie oppofite to each other, and ftop the water ways G and F. In this ftate of the engine, though each of thefe piftons is preffed by a force equivalent to more than a thoufand pounds, they will remain motionlefs, becaufe thefe actions being contrary to each other, they are conftantly in equilibrio. The great pifton H being here fhewn as at the bottom of its cylinder, the tumbler is to be thrown by hand into the pofition here delineated. Its action upon O F, and confequently upon the wheel Q, draws up the plug D, and depreffes E, fo that the water way G becomes open from A B, and that of F to the pipe R: the water confequently defcends from A to C; thence to G G G, until it acts beneath the pifton H. This preffure raifes the pifton, and if there be any water above the pifton, it caufes it to rife and pafs through F into R. During the rife of the pifton, (which carries the pit-rod M N along with it) a fliding block of wood I, fixed to this rod, is brought into contact with the tail K of the tumbler, and raifes it to the horizontal pofition, beyond which it overfets by the acquired motion of the weight L.

The mere rife of the pifton, if there were no additional motion in the tumbler, would only bring the two plugs D and E to the pofition of reft, namely to clofe G and F, and then the engine would ftop; but the fall of the tumbler carries the plug D downwards quite clear of the hole F, and the other plug E, upwards quite clear of the hole G. Thefe motions require no confumption of power, becaufe the plugs are in equilibrio, as was juft obferved.

New engine for raiſing water by the deſcent of a column incloſed in a pipe.

In this new ſituation the column A B no longer communicates with G, but acts through F upon the upper part of the piſton H, and depreſſes it; while the contents of the great cylinder beneath that piſton are driven out through G G G, and paſs through the opening at E into R. It may be obſerved, that the column which acts againſt the piſton is aſſiſted by the preſſure of the atmoſphere, rendered active by the column of water hanging in R, to which that aſſiſting preſſure is equivalent, as has already been noticed.

When the piſton has deſcended through a certain length, the ſlide or block, at T, upon the pit-rod, applies againſt the tail K of the tumbler, which it depreſſes, and again overſets; producing once more the poſition of the plugs D E, here delineated, and the conſequent aſcent of the great piſton H as before deſcribed. The aſcent produces its former effect on the tumbler and plugs; and in this manner it is evident that the alternations will go on without limit: or until the manager ſhall think fit to place the tumbler and plugs D E in the poſitions of reſt; namely ſo as to ſtop the paſſages F and G.

The length of the ſtroke may be varied by altering the poſitions of the pieces T and I, which will ſhorten the ſtroke the nearer they are together; as in that caſe they will ſooner alternate upon the tail K.

As the ſudden ſtoppage of the deſcent of the column A B, at the inſtant when the two plugs were both in the water way, might jar and ſhake the apparatus, thoſe plugs are made half an inch ſhorter than the depth of the ſide holes; ſo that in that caſe the water can eſcape directly through both the ſmall cylinders to R. This gives a momentary time for the generation of the contrary motion in the piſton, and the water in G G G, and greatly deadens the concuſſion which might elſe be produced.

Former attempts to make preſſure engines, upon the principle of the ſteam engine, have failed, becauſe water not being elaſtic could not be made to carry the piſton onwards a little, ſo as completely to ſhut one ſet of valves and open another. In the preſent judicious conſtruction the tumbler performs the office of the expanſive force of ſteam at the end of the ſtroke.

Rate of work by theſe engines.

There are ſeveral other engines of this conſtruction at work. The general rate of working of all theſe engines is, that four feet

feet of water in its descent will raise three to the same height with well made pump work, with the velocity of one foot and a half per second; that is to say, the present engine works ten strokes per minute, and this load is better or more productive, than when either the velocity or the mass are increased.

A much larger engine of the same construction is at work at Trenethick Wood Tin Mine in the parish of Wendron, near Hailstone. It has a cylinder seventeen inches in diameter, and works by a nine foot stroke, ten strokes per minute. The whole fall is thirteen fathom. It has been in constant work nearly three years night and day, with little or no repair, the working parts being so few and simple. This engine must throw up about ten cubic feet per stroke, or 6000 feet per hour, which is about 750 hogsheads. Account of a larger pressure engine.

There is another new engine in the cliff at the Land's-End, to clear the Riblows Tin Mine in the parish of St. Just, which is under the sea. Its cylinder is five inches in diameter, stroke eight feet, and fall twenty fathom.

Mr. Trevithack has erected many single pressure engines, the construction of which I hope hereafter to communicate to my readers. Single pressure engines.

II.

Experiments and Remarks on the Passage of Heat through Fluids downwards, particularly with Regard to the Uncertainty produced by the Vessel; with a Method of obviating that Uncertainty altogether. By JOHN MURRAY, *M. D. Lecturer on Natural Philosophy and Chemistry at Edinburgh. Communicated by the Author.*

Edinburgh, Jan. 20, 1802.

THE opinion which Count Rumford has advanced, that fluids are non-conductors of caloric, may still be considered as admitting of discussion. Though supported by a number of experiments contrived and executed with the greatest ingenuity and address, it is regarded by many chemists as doubtful, whether it is established to the full extent that Count Rumford has stated it, or whether the phenomena attending change of temperature in fluids justify more than the conclusion, Count Rumford's opinion that fluids are non-conductors of heat is doubtful;

sion, that they conduct caloric in a very imperfect degree. From a series of experiments which I have undertaken on this subject, I have been convinced, that investigations respecting it are liable to sources of error more important, and more difficult to be avoided than might *a priori* be supposed, and that we have no experiments sufficiently unobjectionable to decide the question. The observations I shall have to offer in the following memoir, will I trust justify these assertions.

but the experiments offered to decide the question abound with difficulties.

When caloric is communicated to the surface of any solid substance, the temperature of the whole mass is increased with more or less celerity, and becomes uniform. In this case the caloric passes from one particle of the solid to another, and is thus equally distributed through the whole. When caloric is communicated to a mass of fluid, its temperature is likewise uniformly increased, and in this case also it was supposed, that the caloric is transmitted through the fluid, as in the former example through the solid. Count Rumford denies the possibility of such a transmission, and conceives the change of temperature in the fluid to be produced in another mode.

Communication of heat through solids;

and fluids.

When any portion of a fluid is heated it must necessarily be expanded, its specific gravity must be diminished, and from its mobility it must change its place. If therefore caloric be communicated through a solid surface to a fluid, the portion of fluid immediately in contact with the solid, when heated, will acquire a specific gravity different from that of the mass; if the caloric is communicated from beneath, the heated portion of fluid must ascend, its place will be occupied by another portion, and thus the whole mass will be successively brought into contact with the heated surface, and will have its temperature increased. It is in this mode only according to Count Rumford, that the temperature of fluids can be changed. They can receive caloric from any solid matter with which they are in contact, but the heated portion communicates no part of its caloric to the rest of the mass, and the change of temperature which the whole suffers is owing solely to the motions of its parts.

Circulation of fluids from heat.

That it is in a great measure owing to this cause, has been clearly demonstrated by Count Rumford's experiments. He has shewn, that when heat is applied to a fluid, motions of its parts take place, and that whatever retards or obstructs these motions, diminishes the celerity with which the temperature is

The circulation does not *absolutely* prove the Count's opinion.

is changed. It is evident, however, and is admitted by Count Rumford himſelf, that the experiments by which theſe facts are eſtabliſhed, conſidered in themſelves, do not prove the propoſition, that change of temperature in any fluid is ſolely effected by this cauſe, for although theſe motions may accelerate the change, it is ſtill poſſible, it may likewiſe in part be owing to a communication of caloric from one part of the fluid to another.

There is another principle, however, from which Count Rumford's opinion may be eſtabliſhed. It is evident that if it be juſt, a fluid muſt be incapable of being heated downwards. If a hot body be applied to its ſuperior ſurface, the upper ſtratum of fluid will have its temperature raiſed, but it will communicate no part of the caloric it has received to the fluid beneath, and as it cannot change its place, but muſt remain at the ſurface, the under parts of the maſs muſt remain unaltered with reſpect to temperature.

But it might be eſtabliſhed by ſhewing that fluids cannot be heated from above.

It is by eſtabliſhing accurately the fact with reſpect to this point, that the queſtion regarding the conducting power of fluids muſt be determined. Count Rumford has made a number of experiments to prove the propoſition, that caloric is not propagated through a fluid downwards. Of theſe the moſt concluſive ſeems to me to be that in which a heated cylinder of iron was ſuſpended in olive oil, and diſtant only two-tenths of an inch from a projection of ice fixed at the bottom of the veſſel, without any of the ice being melted. On the ſame principle I employed the apparatus, of which a repreſentation is given Fig. I.

Many of the Count's experiments are directed to this proof; particularly that in which an iron cylinder did not melt ice placed beneath olive oil.

The thermometer A is bent ſo that the leg *a*, which iſſues from the bulb is longer than the leg *b*, to which the ſcale is attached. It is ſo far filled with mercury, that at any moderate natural temperature the leg *a* ſhall remain completely filled. At the bottom of *b* the ſcale commences with 20° of Fahrenheit. This bent thermometer is attached to a glaſs rod, inſerted in a circular piece of wood. This is fixed by means of wax to the bottom of a glaſs cylindrical veſſel B, three inches in diameter, and nine in height. With this apparatus, the following experiments were made.

New experiments. Apparatus.

Experiment 1. Into the veſſel B, water was poured till it covered the bulb of the thermometer $\frac{1}{4}$ of an inch; its temperature was 46° of Fahrenheit, which was likewiſe the temperature

I. Heated oil, poured upon water, heated a thermometer through the water downwards.

rature of the air of the room. One ounce of olive oil heated to 140° was poured on a ſmall piece of card, ſuſpended on the ſurface of the water, and the card was ſlowly withdrawn. Any motion of the water was thus avoided. In the courſe of a minute the thermometer began to riſe ſlowly, in five minutes from the commencement of the experiment it had riſen four degrees, in ten minutes $6\frac{1}{2}$, in fifteen minutes eight degrees. It then became ſtationary, and continued ſo for ſeven minutes before it began to fall. Its deſcent was very ſlow.

II. A braſs ball at 212° alſo gave heat downwards.

Experiment 2. Into the veſſel B, water at 49° was again poured, till it covered the bulb of the thermometer one inch. A braſs ball $1\frac{1}{2}$ inch in diameter, was heated to 212 in boiling water, and ſuſpended over the bulb of the thermometer by a wire, previouſly adjuſted ſo as to be diſtant from it $\frac{1}{4}$ of an inch. For $2\frac{1}{2}$ minutes after immerſion of the ball, the thermometer did not ſeem to be affected, in five minutes it had riſen a degree and a half, in ten minutes from the commencement of the experiment $4\frac{1}{2}$, in 15 minutes $7\frac{1}{2}$, in 20 minutes $8\frac{1}{2}$ degrees. It then became ſtationary, and did not begin to deſcend till 15 minutes more had elapſed. By ſuſpending a thermometer repreſented by C in Fig. 1. ſo that its bulb was merely immerſed under the ſurface of the water, the riſe of temperature in the fluid in contact with the bulb was found to be in a few minutes 82, from which point it again deſcended gradually.

It may perhaps be more ſatisfactory to have this latter experiment ſtated more minutely in the form of a table.

Temperature of the air of the room, and of the water in the veſſel 49°.

In 3 minutes after immerſion of the heated ball,

	Therm. A $49\frac{1}{2}$	Therm. C 78
5 min. - - - - -	$50\frac{1}{2}$ -	82
10 - - - - -	$53\frac{1}{2}$ -	73
15 - - - - -	$56\frac{1}{2}$ -	69
20 - - - - -	$57\frac{1}{2}$ -	65
30 - - - - -	$57\frac{1}{2}$ -	62
40 - - - - -	57 -	60
50 - - - - -	$56\frac{1}{2}$ -	$58\frac{1}{2}$
65 - - - - -	56 -	56

On repeating both theſe experiments ſimilar reſults were obtained with variations ſo inconſiderable as not to affect their accuracy.

In

In these experiments then a quantity of caloric was conveyed downwards through a portion of fluid, so as to occasion a considerable rise of temperature in the thermometer, in the 1st experiment to the extent of eight degrees in 15 minutes, in the 2d, 8½ degrees in 20 minutes. From these results, the conclusion might seem just, that the fluid must possess a conducting power. Further investigation, however, will shew that this is not so certain, as at first view it may appear. It is rendered doubtful by the circumstance, that in all experiments of this kind a quantity of caloric must be conveyed by the sides of the vessel. This quantity it is scarcely possible to ascertain with any accuracy, and of course we are unable to determine what share it has in occasioning the rise of temperature, and therefore whether it is the sole cause, or whether any part of that rise depends on the power the fluid has of conducting caloric.

Whether the sides of the vessel were the sole cause of this transmission.

That in these experiments caloric must be conveyed to the thermometer by means of the vessel in which the fluid is confined, may be easily shewn. In the first experiment, when the heated oil was poured on the surface of the water, it came immediately in contact with the sides of the vessel. The glass must of course have been heated, and it would conduct caloric in every direction. The portion of caloric conveyed downwards would again be communicated to the cold water in contact with the sides, this being expanded, and having its specific gravity lessened could not remain touching the vessel, but must have formed a heated current rising from its sides, and extending beneath the oil. A descending current must consequently have been formed in the axis of the vessel, and a stratum of heated water must have been constantly accumulating. The thermometer would thus come to be affected sooner or later, proportioned to the depth of the bulb in the fluid, and it would continue to rise till the temperature of the whole portion of fluid over it became nearly uniform. A similar effect would be produced in the second experiment, as the heated ball would immediately communicate part of its caloric to the portion of water with which it came in contact, and this flowing towards the sides of the vessel, and being constantly succeeded by other heated portions, must have communicated caloric to the glass.

Developement of the manner in which a fluid is heated downwards *by its containing vessel.*

It

It cannot be known whether this be all that happens.

It may be ſuppoſed perhaps, that the quantity of caloric thus conveyed by the veſſel would not be ſufficient to produce the riſe of temperature which took place in theſe experiments. But how is this to be aſcertained. It may appear probable, but until it is proved, the point muſt remain doubtful, and the experiments muſt be therefore inconcluſive.

To ſurround the veſſel externally with water

It was an obvious idea, that this ſource of error might perhaps be obviated in a great meaſure by ſurrounding the veſſel with water, which would carry off the caloric conveyed by its ſides. It is evident, however, that it could carry off only part of it, for part muſt ſtill continue to be abſtracted from the internal ſurface by the water in contact with it. And even the quantity taken from the external ſurface of the jar by the water ſurrounding it, would be only ſomewhat greater than what would be carried off by the aſmoſphere when the experiment was performed as deſcribed above.

promiſed to be of ſome uſe. Experiment. Heat was found to deſcend in the internal and external water.

The experiment however of ſurrounding the veſſel with water might afford ſome information: it would at leaſt prove, that caloric was conveyed by the ſides of the veſſel, and it might indicate in ſome meaſure the quantity conveyed. I accordingly performed it in the manner repreſented in Fig. 2. D being merely a cylindrical glaſs veſſel ſix inches in diameter, in which the apparatus uſed in the former experiments was placed. Water was poured into it, to the ſame height as in the internal veſſel, and a thermometer was ſuſpended in the water, the bulb being at preciſely the ſame diſtance from the ſurface, as the bulb of the bent thermometer in the veſſel A. The braſs ball heated to 212° was ſuſpended in the water in the latter veſſel, and at the ſame diſtance from the thermometer as in the preceding experiments. The following were the reſults. At the commencement of the experiment, both thermometers were at 46°.

In 3 minutes, the internal thermometer	A $46\frac{1}{2}$	the external E ſtill 46
5 min.	$47\frac{1}{4}$	$46\frac{1}{2}$
10	$48\frac{1}{2}$	$47\frac{3}{4}$
15	$49\frac{1}{2}$	$48\frac{3}{4}$
20	$49\frac{1}{2}$	$48\frac{3}{4}$
30	49	48

Many reaſons why this experi-

This experiment proves nothing more, than that a quantity of caloric is conveyed by the ſides of the veſſel. It might perhaps

perhaps be ſuppoſed, that by comparing the riſe of temperature in the two thermometers, a concluſion might be drawn with reſpect to the mode in which the caloric was conveyed to them. If it were intirely conveyed by the ſides of the veſſel, ſince as much ought to be given out by the external as by the internal ſurface, the water without ſhould be heated as much as that within, and conſequently the two thermometers ſhould riſe equally, and to the ſame extent. But if the thermometer in the inner veſſel roſe more than that in the outer, it might be ſuppoſed, that the interpoſed fluid had directly conveyed to it part of the caloric from the heated ball above, with which it was more nearly in contact. But to eſtabliſh ſuch a concluſion from any differences of this kind, certain circumſtances are indiſpenſible, which are abſolutely unattainable. It is requiſite, for example, that both thermometers ſhould be preciſely on the ſame level, at the ſame diſtance from the ſides of the veſſel, and covered with the ſame quantity of water occupying the ſame volume. The ſmalleſt variation in theſe circumſtances would produce a variation in the reſult. But if they were even obtained alike, the ſituation of the veſſels muſt ſtill remain very different; the one for example, preſents a much more extenſive ſurface to the ſurrounding medium than the other, and the nature of that medium is different, the internal veſſel being ſurrounded with water, the temperature of which is augmenting while the experiment continues, the external with atmoſpheric air, whoſe temperature remains the ſame. It is impoſſible to eſtimate the differences in effect which muſt ariſe from theſe differences in ſituation, and conſequently no concluſion can be drawn from the comparative alterations of temperature in the reſpective thermometers, as to the quantity of caloric conveyed to either of them by the ſides of the veſſel.

ment cannot be made deciſive.

The preceding experiments then, or any of a ſimilar nature, are incapable of determining the queſtion reſpecting the conducting power of fluids. In all of them a quantity of caloric is conveyed by the ſides of the veſſel in which the experiment is made, and this quantity we have it not in our power to aſcertain, ſo as to determine whether it is adequate or not to produce the augmentations of temperature which actually take place, and conſequently whether any part of that augmentation is to be aſcribed to a conducting power in the fluid.

The ſides of the veſſel will always conduct heat;

and no precaution can infure the object of the experiment.

Every precaution that can be taken to obviate this fource of error, defeats the purpofe of the experiment itfelf, fince fuch precautions tend equally to leffen the effect which would refult from a conducting power in the fluid fuppofing it to poffefs it, and it muft therefore remain uncertain to what the diminifhed effect which takes place where they are employed, is to be attributed.

Other remarks.

If for example, the bulb of the thermometer be placed at a great diftance from the matter communicating caloric, it will be a longer time before the thermometer begin to rife, and it will alfo not rife to fo great a degree. But which ever opinion be adopted, whether that the fluid directly conducts caloric, or that caloric is communicated only by the veffel, this ought to be the cafe, fince the circumftances neceffary for its tranfmiffion in either way are rendered more unfavourable.

In like manner when a wider veffel is employed to contain the fluid, the rife of temperature muft be lefs than when a narrow one is ufed, becaufe in this cafe alfo there muft be a larger portion of interpofed fluid to be heated.

Or when a large portion of fluid is placed above the bulb of the thermometer, and in contact with the fubftance communicating the caloric, as with the ball in the preceding experiments, the fame diminution of effect muft take place, becaufe the caloric given out by the ball being abforbed by a larger quantity of fluid, the temperature of that fluid muft be lefs increafed, and it muft have lefs effect either in heating the fides of the veffel, or the thermometer.

General obfervation or refult.

It appears therefore that in all experiments of this kind, a fource of fallacy muft be prefent: the effects of this will be more or lefs confiderable according to the circumftances of the experiment, but they cannot be intirely obviated, nor their extent appreciated, fo as to admit of a certain conclufion being drawn.

Hiftory of the experiments.

It may not be improper to remark, that the preceding experiments were made laft winter, were ftated in my courfe of lectures, and a concife account of them publifhed in my Elements of Chemiftry. I have now ftated them partly to fhew the infufficiency of experiments of this kind, either to eftablifh or controvert Count Rumford's opinion, but principally as an introduction to thofe I have further to relate, and which appear to me to be free from the fource of error I have ftated.

This

This, of which I at one time defpaired, may be attained by the fimple contrivance of employing a veffel of ice. If a cylindrical veffel of ice be procured fimilar to that ufed in the preceding experiments, and if a thermometer be fixed in it, if on filling it with a fluid at the temperature of 32°, and fufpending in it, at a fhort diftance from the bulb of the thermometer, a heated folid, any rife of temperature take place, it may be confidered as a certain proof of a conducting power in the fluid. No caloric could poffibly be conveyed to it by means of the veffel, fince ice cannot have its temperature above 32°, and there is no other mode by which the caloric can pafs from the ball to the thermometer than by the interpofed fluid. Of the experiments made to determine this point, an account will be given in a fubfequent memoir.

A veffel of ice at 32°, containing water at 32° will not raife the temperature of the fluid during its own fufion.

III.

Letter from Dr. Van Marum *to Mr.* Volta, *Profeffor at Pavia, containing Experiments on the Electric Pile, made by him and Profeffor* Pfaff, *in the Teylerian Laboratory at Haarlem, in November,* 1801. *

THE weather and feafon of the year preventing thefe gentlemen from attempting to charge the whole of the great Haarlem battery, confifting of 100 jars, in general nearly a line thick, and containing five fquare feet and a half of coating each, with the galvanic pile, they took five-and-twenty of thefe jars, which they charged feparately, a few at a time, and all together; and uniformly found the fingle jars or the batteries charged to the fame degree of intenfity as the pile. They had taken twenty-fix jars, but one of them did not receive the charge well, which they afcribe to the too great thicknefs of the glafs.

Van Marum's experiments on the galvanic pile.

They next charged the battery of $137\frac{1}{2}$ fquare feet with a greater or lefs portion of the pile, by foldering a hook to every twentieth plate of zinc, to which the infulated metallic wire for producing the communication between the battery and the pile might be conveniently attached. Beginning at the

Battery of $137\frac{1}{2}$ fquare feet charged by it,

* Abridged from the Annales de Chimie, No. 120, Vol. XL. p. 289.---C.

taking its electricity at different heights.

the fortieth pair from the bottom, the firſt where the ſeparation of Bennet's electrometer was diſtinctly perceptible, the battery was charged to the ſame intenſity as the electrometer indicated when brought into contact with that part of the pile, giving here a ſeparation of one line. The effect was preciſely analogous when the battery was charged by ſimilar inſtantaneous contact with the pile at the ſixtieth, eightieth, or hundredth pair, and ſo on.

The zinc being at top, the electricity poſitive.

In this pile, the ſilver being the lowermoſt of the metals, the electricity was poſitive at top, and the electricity communicated to the interior ſurface of the battery was the ſame, the contact being made with the ſuperior part of the pile.

The metals being inverted, the electricity charged.

The order of the metals was then inverted, and the experiments repeated at different heights of the pile as before, with ſimilar reſults.

Shocks taken by copper conductors two inches in diameter from the battery thus changed.

They now proceeded to examine the ſhocks given by the battery charged at different heights of the pile; for which purpoſe they employed two conductors of copper, two inches in diameter, held in wet hands. Beginning with the battery charged at the twentieth pair, they very diſtinctly perceived the paſſage of the ſtream from the conductor into the hand, and from the hand into the conductor. One gentleman preſent, Mr. Vander Ende, felt it as far as the wriſts. When the battery was charged by forty pairs, real ſhocks were felt at the wriſts: when by ſixty, the ſhocks were very perceptible at the elbows: and they gradually increaſed, till the ſhocks extended to the ſhoulders with conſiderable ſtrength, when the battery was charged by the whole pile.

The ſhocks given by the pile itſelf much ſtronger.

Theſe ſhocks, however, were not equal to thoſe given by the pile itſelf, but only about half their ſtrength; the ſhock from a hundred pairs appearing to be equal to that of the battery when charged by two hundred.

Comparative experiments made with an electrical machine.

Having continued theſe experiments till nothing more could probably be learned from them, the comparative effects of the electrical machine, conſiſting of a plate of glaſs thirty-one inches in diameter, were tried with the ſame battery.

Precautions uſed in conducting theſe experiments.

This experiment requiring particular precautions, that the contact of the conductor might not impart more electricity to the battery than was furniſhed by the action of the machine during the contact, Dr. Van Marum formed a communication between the conductor and the ground by touching it with one

one of his fingers, and withdrew his finger the moment he brought the large wire into contact with the conductor. This manœuvre he practiſed repeatedly, till he had acquired a habit of doing it with preciſion, before he began his comparative experiments; ſo that he was well aſſured, that the contact of the conductor, by means of the wire, took no more of the fluid than was furniſhed by the machine at the inſtant of contact. A ſingle contact would not give the battery a change, that the electrometer was capable of indicating; but by repeated experiments it was found, that ſix of theſe contacts of the conductor imparted to the battery the ſame intenſity as one contact of the pile. The power of the machine being equal to about half what the great machine poſſeſſed in its former ſtate, from 1785 to 1789; but the great machine having gained conſiderably, particularly for charging batteries, by the rubbers of the new conſtruction applied in 1790, and by the uſe of Kienmayer's amalgama, ſo that its power was rendered the quintuple of what it was before; the power of the pile of 200 pairs is to that which the great Teylerian machine poſſeſſes at preſent as 3 to 5. The ratio could not be examined directly, becauſe a fire cannot be made in the muſeum, and the ground on which it ſtands is very damp in winter: this, therefore, is deferred till ſpring.

The pile of 200 pairs had ſix times the power of the machine with a plate 31 inches in diameter,

and three-fifths of that the great Haarlem machine.

The ſhocks given by the battery, charged to different degrees of intenſity by contacts with the conductor of the electric machine, were now compared with thoſe it gave with ſimilar intenſities obtained from the pile; and reiterated trials convinced the experimentaliſts, that there was no perceptible difference between the ſenſations or ſhocks in the two inſtances, provided the intenſities were the ſame. Hence Dr. Van Marum concludes, as the effects of ſuch a conſiderable battery, when charged by the galvanic pile, are preciſely ſimilar, in every reſpect, to its effects when charged by a powerful electrical machine, the identity of the fluid put in motion by the pile with that moved by the electrical machine is proved ſo deciſively, that no one will queſtion them in future. Theſe experiments, he adds, joined with thoſe of Profeſſor Volta, render extremely diſputable, or rather completely refute the exiſtence of a peculiar fluid, in all the other experiments termed galvanic.

The ſhocks given by the battery, charged by the machine, not at all different from thoſe it gave when charged by the pile to the ſame height:

the two fluids therefore *preſumed* to be the ſame.

Theſe

Extreme velocity of the fluid.

These experiments have farther proved in a decifive manner, and on a large fcale, another important circumftance refpecting the pile; namely, that the ftream of fluid moved by it has a velocity furpaffing all conception; fince a battery of $137\frac{1}{2}$ fquare feet was charged to the intenfity it difplayed by a contact as fhort as poffible with the wire of communication, a contact that did not continue one-twentieth of a fecond.

Hence the effects of the pile fuperior to thofe of common electrical machines;

After this it is no way aftonifhing, that the pile, by a continuance of its action, fhould produce fuch effects as have never been feen where common electrical machines are employed, as the fpeedy decompofition of water for inftance. Certainly no other electrical machine known, the grand Teylerian machine excepted, can furnifh a continued ftream at all approaching that of the pile; on which account it is a powerful mean of producing feveral effects, which may contribute greatly to the progrefs of natural philofophy.

and may be of great ufe in phyfics.

Method of infulating the pile.

This confideration induced Dr. Van Marum to endeavour to augment the power of the pile. From the beginning he was careful to infulate it more effectually than is commonly done. For this purpofe he placed it on a thick cake of lac, and kept it in its vertical pofition by fticks of fealing-wax two inches long, fixed horizontally in four flips of wood, furrounding the pile. Thefe fticks of fealing-wax were fixed to little wooden cylinders, paffing through holes in the uprights at every four inches, and kept in their places only by being made to fit with fuch a degree of tightnefs, as would allow them to be moved backward or forward, fo as to fuit piles of different diameters. The uprights were fixed at top and at bottom in a piece of wood a foot fquare. Profeffor Pfaff himfelf was aftonifhed at the great effect of the firft pile compofed of three Guilder pieces, plates of zinc of the fame diameter, being an inch and a half, and bits of cloth moiftened with a faturated folution of muriate of ammoniac. In one of the experiments the end of an iron wire, No. 16, $\frac{2}{240}$ of an inch in diameter, was made red-hot for the length of a line, and even fufed at the extremity, by a pile of fixty pairs.

Wire fufed by it.

Having read an account of the experiments of Fourcroy, Vauquelin, &c. on the fufion of wire by large plates of copper and zinc, Dr. Van Marum procured thirty-two of each metal, five inches fquare, and made a pile with them; firft of

of ten inches in diameter, and eight pairs in height, by placing four of each metal together as one plate, they being exactly square, and their surfaces very smooth, that they might be in close contact; and then of five inches in diameter, and thirty-two pairs in height. The power of the higher pile greatly exceeded that of the larger. In August, Dr. Van Marum had fused completely into globules five inches of the iron wire mentioned above, and made seven inches red-hot, with this pile.

Pile of ten inches in diameter.

Another of five.

The powers of piles depend more on their height, than on their diameter.

The number of pairs of metal being increased, the effect was not; fifty pairs producing less than twenty-five, by which nine inches of the same wire were melted when Professor Pfaff was present in November. This they ascribe to the moisture being too much pressed out of the pieces of pasteboard by the weight of so many plates of metal a line and a half or two lines thick; as the upper twenty-five pairs, being taken off, had as much effect as before, while the lower twenty-five had not half the power. In consequence they divided the pile into four, containing together 110 pairs. The plate of copper, placed under and connecting the two piles that were insulated, had a rim, that the solution pressed out of the pasteboard might remain in it, and not wet the insulating cake, and destroy the insulation. These two piles, containing sixty pairs somewhat thinner than the others, when not connected with the other two, made six inches of the wire red-hot. The other two piles, containing fifty pairs, made eight inches very red, and fused great part of this length. This difference was ascribed to the pasteboards not being sufficiently moistened in the former instance. The four piles connected together made twelve inches of the wire red-hot.

Effects not increased in proportion to the height, when the plates are large and thick, because too much fluid is pressed out.

On this account the pile should be divided into several, communicating with each other.

On taking sparks repeatedly at each extremity of the connected piles, by means of a wire communicating with a basin containing quicksilver, no difference between those of the positive and those of the negative electricity could be discerned. Whether issuing, or entering, they appeared radiating, when iron wire was employed; which the experimentalists ascribe to the combustion of the iron, for when a wire of platina was used, no rays were perceptible in the sparks of either electricity. Sometimes the communication was made by means of a needle, fastened to the slender wire

Positive and negative sparks did not differ.

which was fixed to the end of the conducting wire; at other times by the leſs pointed end of the conducting wire itſelf.

Beautiful phenomena produced by the combuſtion of of the wire.

When the ſurface of the quickſilver was touched with the end of a ſlender wire inſtead of the needle, a very ſtriking phenomenon enſued: the combuſtion of the extremity of this wire was effected with ſuch force, that it threw out ſparks on all ſides, which formed thouſands of apparent rays, repreſenting beautiful ſuns ſome inches in diameter. By gently lowering the end of the wire in proportion as it was diſperſed by the combuſtion, this appearance could be continued at will. It is ſeen in ſome degree likewiſe when the quickſilver is touched with the point of a fine needle; but then it is of ſhort duration, as it ceaſes as ſoon as the needle has left its point. The experiment was repeated with wires of $\frac{1}{240}$, $\frac{1}{151}$, and $\frac{1}{105}$ of an inch in diameter: that of the middle ſize gave the largeſt and moſt brilliant ſuns, but the ſmaller ſucceeds better when the power of the pile is leſs.

Quickſilver oxided on its ſurface by the ſparks.

On taking wires too thick to be melted, the oxidation of the quickſilver at its ſurface by each ſpark was more diſtinctly ſeen, ſpots of upward of a line in diameter being formed.

Platina fuſed by the pile.

The extremity of a wire of platina, about $\frac{1}{175}$ of an inch in diameter, was melted, and formed a globule.

Sparks upward of a line in diameter.

The ſparks iſſuing from the end of the communicating wire, when it was not too ſlender, were more than one-tenth of an inch in diameter.

Piles of an equal number of pairs, though different diameters, give equal intenſities, and ſimilar ſhocks; notwithſtanding they differ ſo much in fuſing metals.

On comparing the intenſity of the electricity of this pile with that of the former of zinc and ſilver an inch and a half in diameter, both by the ſimple contact of the Doctor's moſt ſenſible electrometer, and by the condenſer and an electrometer of leſs ſenſibility, they were found preciſely the ſame, when the ſame number of pairs were uſed: they charged the battery before deſcribed to the ſame height: the ſhocks given by the battery thus charged were in both caſes preciſely the ſame: and in thoſe from the piles the difference was ſcarcely perceptible.

Attempt to account for this.

It appears ſtrange, that two piles ſo equal as to intenſity, and charging large batteries in ſuch an equal degree, ſhould produce ſuch different effects in fuſing metals. To form a juſt notion of this, obſerves Dr. Van Marum, we muſt diſtinguiſh the action of a pile which is inſulated from that of one which is not. In the latter there is a ſtream paſſing continually from

from one extremity to the other, and through the conductor or chain of conductors which connects the two extremities: but this stream cannot take place in an insulated pile. From this single distinction it appears, that the equality of the intensities of insulated piles affords no reason to expect an equality of the streams of the two piles when not insulated: on the contrary, if the stream meet with less impediment in a large pile, than in one of smaller diameter, the stream will produce a greater effect, in consequence of its greater velocity.

Effects of different sized pasteboards.

To see how far this velocity depended on the size of the pieces of pasteboard employed, two piles were constructed, each of twelve pairs of the large plates, in one of which the pasteboard was cut to the size of the plates, in the other to the diameter of half an inch only. The intensities of both were the same, yet the other effects were much greater in the pile with large pasteboards; for it gave very brilliant sparks, which began with the fifth pair from the bottom, while the other pile scarcely emitted any that were perceptible. On making the pasteboards somewhat larger than the metallic plates, the effect was by no means increased, but rather diminished. It was found necessary, likewise, that the pasteboards in the large pile should be well wetted.

These should not be too large: and should be well wetted.

Anomaly not accounted for by the hypothesis.

There is another apparent anomaly, however, in the effects of piles differing in diameter: though the action of the large pile is so much more powerful in fusing metals, not the least difference can be perceived in the shocks they give: and this Dr. Van Marum confesses is not accounted for by his hypothesis.

Solution of muriate of ammonia preferable for moistening the cloths.

Some farther experiments were made respecting the efficacy of the solution of muriate of ammonia, which was found to be preferable to common water, or to a solution of muriate of soda, for moistening the cloth or pasteboard. A pile of twenty pairs with the ammoniac solution fused into globules four inches of wire, No. 16; while that with solution of common salt did not fuse or even redden a single line, and that with simple water shewed still less effect with regard to the sparks it produced.

Not because it oxidates the metals, for it is said to be supposed

As several authors have ascribed the greater effect produced by saline solutions to the oxidation of the metals, many experiments were tried, with a view to ascertain this point,

 by

rior to the more powerful acids.

by moiſtening the paſteboards with ſulphuric, nitric, and muriatic acids, both in a concentrated ſtate, and more or leſs diluted. The particulars are not given, but the reſults are ſaid not to have favoured the ſuppoſition, on a compariſon of the intenſities, ſhocks, or ſparks; all of which were ſtronger when muriate of ammonia was employed, than when nitric acid was uſed, either concentrated or diluted, by which the two metals was very ſpeedily oxidated. As nothing is ſaid here on the fuſion of wire, this does not appear to have been tried.

Ammonia alone not equal to the muriate.

Ammonia alone being uſed to moiſten the paſteboards, the effects produced were all leſs than with its muriate.

Effects of the pile in the air, in vacuo, in carbonated hidrogen gas, and in azote gas, equal:

Experiments were likewiſe made in vacuo, and in different airs. The apparatus employed very readily produces a vacuum in which the barometer falls to below one line; but in this inſtance the vapor produced by the water of the pile kept the quickſilver at the height of five lines. Between the effects produced in the open air, in this vacuum, in carbonated hidrogen gas, and in azote gas, no difference could be diſcovered. On the introduction of pure oxigen gas, however, the ſparks were much larger, more brilliant, and eaſier to be obtained: but on making a vacuum after this, the ſhocks were feebler, and the ſparks ſmaller, than they had been in any of the former inſtances. Oxigen gas being introduced a ſecond time, its effects were as powerful as before: the cylinder being emptied again, the effects of the pile were reduced as much as before: and on letting in atmoſpheric air they were reſtored to the ſame degree nearly as in the former experiments.

but in pure oxigen gas much increaſed.

A pile with potaſh was very ſtrong, but ſhewed no oxidation.

An experiment of the Doctor was directed to aſcertain the preſence of oxidation in the uſe of a very concentrated ſolution of potaſh in a pile of twelve pairs of five inches ſquare, which produced much more effect than another ſimilar pile with water only. But on ſeparating the pile neither the copper nor the zinc had ſuffered any injury in their poliſh. The Doctor does does not ſay how long the pile ſtood together.

Very large pile of five inches ſquare.

Laſtly, The galvanic apparatus was increaſed to 200 pairs of zinc and copper in ſix connected piles of five inches ſquare each. With this he fuſed an iron wire, No. 16, of twenty-three inches entirely into globules, and ignited the whole of another piece of thirty-three inches.

The

The shock was tried of a column of twenty pairs of copper and zinc of one inch and a half, and of another of the same number of plates of five inches; and also of another of ten inches square; but not the least difference could be perceived. The last mentioned pile (which, as well as the others, was moistened with muriate of ammonia), fused five inches of wire; and the other of five inches square fused four inches. Whence it seems to follow that the effects of these piles, as to the fusion of wire, do not increase in the proportion of their surfaces, but in a less ratio.

The shocks not at all increased by the increase of surface.

Fusion of wire does not increase as the surface, but in a less ratio.

IV.

A Reply to Mr. CRUIKSHANK'S *Observations in Defence of the New System of Chemistry, in the Philosophical Journal* *. *By* JOSEPH PRIESTLEY, *L. L. D. F. R. S. &c. Communicated by the Author.*

HAVING proposed to philosophers the re-consideration of the doctrine of *phlogiston*, which for some time has been almost universally exploded, I am happy to find so truly ingenious and candid a person as Mr. Cruikshank has given some attention to the subject. That experiment of mine which he particularly examines, is that in which I procured a very large quantity of heavy inflammable air from finery cinder and charcoal, both previously exposed to such a degree of heat, as would have expelled from them all the air that mere heat could expel. This I ascribed to the water in the finery cinder uniting with phlogiston from the charcoal. Mr. Berthollet thinks, that this inflammable air comes from the decomposition of the water contained in the charcoal, and Dr. Woodhouse, from that which he allows to be retained in the finery cinder. But Mr. Cruikshank, not satisfied, I presume, with either of these hypotheses, has suggested a very different one, for he asserts, that water is not at all necessary to the production of this inflammable air, maintaining that metals, and their calces, in a very high temperature, have the power of decomposing fixed air, and in this case the fixed air must be formed from the oxigen in the finery cinder, and the carbon in the charcoal.

Short preface and recapitulation.

The heavy inflammable air from finery cinder and charcoal, ascribed to water either simple or decomposed;

but by Mr. Cruikshanks to a decomposition of carbonic acid.

* Vol. V. Quarto Series, page 1.

After

His experiments with metallic oxides and charcoal, in proof,

After repeating my experiment, which he found to be juſt, Mr. Cruikſhank did the ſame with the calces of other metals, particularly thoſe of zinc, copper, lead, and mangancſe, and then concludes, p. 4, that in all theſe caſes "the air muſt "come from the partial decompoſition of the carbonic acid by "the calx when raiſed to a high temperature." But the inference that I think is more naturally drawn from them is, that all theſe calces contain much water, and little or nothing elſe. This I have ſhewn to be the caſe with reſpect to ſeveral of them, eſpecially that of zinc; though I doubt not but that ſome ſmall portion of oxigen may be contained in them all. Indeed, we cannot abſolutely ſay, that any ſubſtance whatever can be wholly expelled from any other, with which it has been chemically combined by any proceſs.

are inferred by Dr. P. to prove that the oxides contain water.

Iron or its oxide heated by the ſolar focus in carbonic acid is ſtated to have produced no decompoſition;

Before Mr. Cruikſhank admitted that iron, or its calx, when raiſed to a high temperature, can decompoſe carbonic acid (*i. e.* fixed air) in this experiment, he ſhould have tried whether it would do it in any other. If in any caſe, I ſhould think it would do it when it was heated in this air by a burning lens, by which a greater heat may be produced than in any open fire. But this I found not to be the caſe either with iron, or this calx of it. In the laſt ſummer I went through a courſe of experiments with this view; but I always found fixed air not to be decompoſed by this means. Though I found that a portion of this air, and alſo of all the other kinds that are readily imbibed by water, was rendered immiſcible in water by means of heat reflected either from a calx of any metal, a piece of earthen crucible, or any other ſubſtance, on which I threw the focus of the lens when it was ſurrounded by this kind of air confined by mercury or water: This, however, was no decompoſition of the air, as there was no oxigen found in it after the proceſs. The addition of permament air was always phlogiſticated.

for though part was rendered immiſcible with water by thus heating either an oxide or a piece of earthen crucible in it;

yet there was no addition of oxigen, but only azote.

Mr. C. produced heavy inflammable gas from iron filings and chalk heated;

Mr. Cruikſhank thought, that if this heavy inflammable air came from the decompoſition of the carbonic acid by the iron, he ſhould ſucceed better, p. 4. by employing iron filings in the place of finery cinder, as they would have a greater affinity with oxigen; and with this view he heated them together a quantity of common chalk, previouſly expoſed to a low heat, for ten minutes. From this mixture he procured a great quantity of air, and he thought that the acid (*i. e.* fixed

air

air from the chalk) was decompofed by the iron; whereas when he ufed well burned lime he got little or no air. What I infer from this experiment is, that the chalk, not being perfectly calcined, contained fome water, as well as fixed air, and that this water uniting with the phlogifton of the iron formed the inflammable air that he found. Water I fuppofe to be the bafis of all the kinds of air, and many fubftances retain it in any degree of heat. Chalk I have found to do it after long expofure to the heat of a fmith's forge.

which the Dr. does not infer to be carbonic acid decompofed; but water with phlogifton from the iron.

Admitting the fixed air procured in the experiment with the finery cinder and charcoal to come in part from the oxigen in the finery cinder, how is this oxigen to be expelled from the calx, fince heat will not do it? And there is no inftance, I believe, in chemiftry, in which when heat alone will not expel any conftituent part of a fubftance, it can be effected without the aid of an affinity, in confequence of which fome other fubftance takes its place. But here, according to the new theory, nothing is fuppofed to take the place of the oxigen in the finery cinder. It takes nothing from the charcoal, but the iron is revived by the mere expulfion of the oxigen.

If the carbonic acid, from finery cinder and charcoal, derive its oxigen from the oxide, this principle muft, it is ftated, be transferred contrary to the laws of affinity.

Mr. Cruikfhank lays great ftrefs on the difference that he found in the air that he procured in thefe proceffes from that which is got from charcoal and water. But I have obferved, that there is a confiderable difference in the qualities of heavy inflammable air, not only according to the fubftance from which it is procured, but in the fucceffive ftages of the fame procefs. He will find that I have examined this kind of air as procured from a great variety of fubftances, made to pafs in the form of vapour through hot earthen tubes, and in various other ways, and have given the analyfis of them. I always found that the firft portions from charcoal were loaded with fixed air, but that in the courfe of the procefs this difappeared, the air burning with a lambent flame, and that towards the end it approached to the explofive kinds, as obtained from metals by acids.

The difference between this gas and that from mere water and charcoal, ftated to afford no conclufive argument.

I alfo found that more or lefs fixed air is procured by the decompofition of heavy inflammable air by means of dephlogifticated air; and though the air procured from finery cinder and charcoal fhewed no fign of its containing any mixture of fixed air, nothing of the kind being difcoverable by lime water;

Oxigen combined with heavy inflammable air affords carbonic acid, in greater quantity than the latter; which

is inferred to have afforded phlogiston.

water; yet when it was decompofed I found much more than the weight of the air; fo that it could not have been previoufly contained in it in a ftate of folution, but muft have been formed by the union of the oxigen in the dephlogifticated air, and the phlogifton in the inflammable air.

Charcoal fuppofed to contain carbon and phlogifton.

That charcoal uniting with water fhould give fixed as well as inflammable air, I account for by fuppofing, what is by no means improbable, that this fubftance contains the elements of both the kinds of air, and that they want nothing but water to enable them to take the form of air.

Conclufion.

I hope that Mr. Cruikfhank, with the fame candour with which he has began this difcuffion, wlll re-confider his hypothefis, and extend his examination to my other arguments in defence of the doctrine of phlogifton, and againft the decompofition of water. Nothing but free difcuffion is neceffary to the difcovery of truth, and it is defirable that error fhould be detected as early as may be, efpecially if the confequence of its reception be extenfive and important.

Your's fincerely,

J. PRIESTLEY.

V.

A Statement of the Experiments made by the Rev. ABRAHAM BENNET, *F. R. S. on the Electricity produced by the contact of Metals previous to the Year* 1789, *and alfo of thofe made by Mr.* TIBERIUS CAVALLO, *F. R. S. previous to the Year* 1795, *to which Allufion was made at Page* 144 *of this Journal.* W. N.

Short ftatement of *Bennet's* experiments, and why they are here repeated.

SOMETIME previous to the period firft mentioned in the title to the prefent fketch, Mr. Bennet had remarkably increafed our power of meafuring fmall intenfities of fingle electricity by the application of gold leaf to the bottle electrometer of Cavallo, and by the procefs known by the name of doubling; an outline of the hiftory of which may be feen at page 396 of the firft volume of the Philofophical Journal, in quarto. His *New Experiments on Electricity,* which is a thin quarto of 141 pages, were publifhed in 1789 by fubfcription; and may perhaps have been lefs univerfally diffufed in the fcientific

ſcientific world, than if the book had appeared in the uſual mode of publication. At all events, as the work is now ſcarce, and the ſubject of ſome intereſt ſo far as it may be thought connected with the theory of galvaniſm, I think it will not be unacceptable to my readers to relate what he did at that early period. The ſame reaſons will in part apply in favour of a conciſe account of the experiments of Mr. Cavallo.

Uncertainty of the electrical doubler; from ſpontaneous electricity,

Soon after the publication of Bennet's doubler in the Philoſophical Tranſactions for 1787, he found that the inſtrument produced electricity without previous communication, and that it always retained that property, whatever care might be taken to deprive it of any adhering charge. When he was afterwards engaged in a courſe of experiments facilitated by the mechaniſm I applied to the doubler in 1788, he found that a very great portion of this adhering electricity might be removed, by turning the handle of the doubler a conſiderable number of times, while all the plates were connected with the earth, and that by virtue of this proviſion the inſtrument might be truſted to indicate the nature of communicated electricity, to a degree of accuracy far exceeding that which could be afforded by any ſimpler inſtrument.

removed by working its parts uninſulated.

Capacity of bodies for electricity ſuppoſed to increaſe by approximation.

By reaſoning upon this phenomenon, he was induced to conjecture, that this ſpontaneous electricity was not owing to accidental friction, bnt to what he called the increaſed capacity of approximating parallel plates, which might attract and retain a charge, though neither of them were inſulated. The experiments he made in ſupport or proof of this hypotheſis were the following:

Experiments. The doubler ſooner exhibited electricity when cleared and left with its plates oppoſite, than when at a diſtance from each other.

He repeatedly tried the effect of depriving the doubler of ſpontaneous electricity, by turning the revolving plate forty times, with braſs wires hooked to all the plates, and he found that if he took off the wires while the revolving plate ſtood at a diſtance from the two ſtationary plates, it was more completely deprived of electricity, than if the wires were taken off when the revolving plate ſtood parallel to the plate A. That is to ſay, it required 21 revolutions to exhibit ſpontaneous electricity after the proviſion in the firſt caſe, and 16 revolutions in the ſecond caſe. Whence it ſeemed to him, that the two plates ſtanding parallel to each other, had by an increaſe of capacity acquired a certain ſmall charge, which

ſooner

fooner rofe by doubling to a perceptible quantity, than that electricity which they might have poffeffed in the firft cafe.

An infulated copper plate placed in contact with a table, with a large approximating furface became negative.

But to make the experiment in a more direct manner, ufing the doubler only as an inftrument of admeafurement, he took a copper plate thirteen inches in diameter, having its furface rather convex, furnifhed with an infulating handle of oiled glafs four inches long, and baked till the whole was well hardened. One end of this glafs was fixed into a copper focket in the middle of the plate, and the other end into wood, that it might not be neceffary to touch the electric part of the handle. This copper plate was placed flat upon a mahogany table, and the doubler being deprived of its electricity, the revolving plate B was placed parallel to A, fo that B was connected with the earth; then the copper plate was lifted up by its infulating handle, and applied to the plate A. And laftly, the plate B being revolved only five times, caufed the gold leaf of the electrometer to diverge negatively to the diftance of a quarter of an inch.

The fame refult when a furface of water was ufed inftead of the table.

It might in this inftance eafily have been ftated, that the contact of the copper plate with the mahogany table, did in fact produce electricity by excitation or friction. But to obviate this, he firft repeated the experiment, by touching the copper plate with the point of a needle, and then applying it to the doubler as before, and he found that the inftrument did not produce its fpontaneous electricity in lefs than 15 revolutions; after which touching the copper plate again with the needle, he applied it to the furface of fome water contained in a large difh, fo that its convex part touched the fluid; and then lifting it up he applied it to the doubler, and electricity was communicated, fufficient to caufe the gold leaf to diverge negatively at five revolutions, as in the former experiment.

When the touch was reverfed the oppofite power was obtained.

The laft experiment with the water was repeated, with this difference only, that the application of the copper plate was made to the revolving plate of the doubler, inftead of the plate A, the revolving plate being moved a little paft its contact with the ball, and the plate A being at that time made to communicate with the earth by the brafs wire; five revolutions in this cafe produced electricity as before, but the electricity of the plate A was now pofitive, as might be expected in confequence of the negative charge having been communicated to B inftead of A.

The

The foregoing experiments were frequently repeated, but as the charge had hitherto been negative, he was defirous of knowing, whether a variation of the nature of the furface might not alter it. He therefore covered the furface of the copper plate with a mixture of gum water and minium, and alfo with gum water and wheat flour, and he found that this fubftance when dried upon the furface of the copper, changed the nature of the charge communicated to the plate to which it was applied.

When the plate was covered with gum water, &c. and dried, its electricity became pofitive.

Laftly, to render what he called the electricity of the approximating plates more confpicuoufly fenfible, he ground a brafs plate three inches diameter with emery, till it would adhere to the furface of a piece of black marble. This plate and marble therefore conftituted a condenfer in its original ftate. The marble being moderately warmed, he preffed the brafs plate upon its furface with the point of a brafs wire; then lifting it up with its infulating handle he applied it to the cap of the electrometer, which caufed the gold leaf to ftrike the fide negatively.

The condenfer affords electricity.

Hence he thinks it muft appear evident, from the precautions and experiments he has ftated, and from the known laws of electricity. 1. That the doubler may be deprived of accidental or communicated electricity. 2. That the principal caufe of its fpontaneous charge, is the attraction of electricity by the approximation of its parallel plates. 3. That this charge may be pofitive or negative, according as the plates or touching wires are compofed of fubftances which have a greater or lefs adhefive affinity with the electric fluid. 4. That the caufes of fpontaneous electricity are common to the condenfer, both in its original and improved ftate, and the doubler, and equal in them all, as far as they are equal in their dimenfions and powers. 5. That fince the doubler may be compofed of very fmall plates, and yet its power be equal to that of a very large condenfer, its fpontaneous electricity will be more eafily overcome by a communicated charge than that of a condenfer of equal power, and therefore experiments performed with it will be lefs liable to equivocal refults.

Enumeration of refults. 1. The doubler cleared of electricity. 2. Caufe of fpontaneous charge. 3. and its nature. 4. Common to all condenfers. 5. The doubler preferable.

This author proceeded to make other experiments, on the fo termed adhefive electricity of metals, and other conducting fubftances. He deprived the doubler of its fpontaneous charge, and placed the two plates A and B oppofite each

Other experiments of adhefive electricity; with iron and fteel;

each other, but so that B was not connected with the earth. He then touched the plate A with the blade of a knife, and the plate B at the same time with the point of a softened iron wire. With sixteen revolutions the gold leaf diverged about one third of an inch positively. The doubler was then again deprived of electricity, and the revolving plate B placed as in the last experiment. The knife was applied to B instead of A, and the soft iron wire to A instead of B, which opened the gold leaf negatively at fifteen revolutions. These experiments were repeated very often, and the electricity changed each time; being always positive in the plate touched with the knife. He remarks, that it would appear incredible, that so minute a difference of adhesive electricity as that which might be supposed between two metals so nearly alike as hardened steel and soft iron could be distinguished, had not the frequent repetition of experiments confirmed it.

with other substances.

He proceeded to make similar experiments, which are tabulated, of different substances, namely, lead ore and lead; lead and iron wire; lead ore and iron wire; tin foil and iron wire; zinc and iron wire. All the experiments having been made by double contact and alternate application of the opposite substances to the plates A and B, he also by single contact determined whether the adhesive electricity, or to state the fact more unexceptionably, the electricity produced by each substance in the doubler, was positive or negative. I have thought it less immediately necessary to state the particulars of these experiments, because I presume that the substances were held in the hand, and from a variety of facts, I think we are justified in concluding that friction, or a contact equivalent to friction, between the human skin and the substance thus held, will produce electricity more than sufficient to render it unnecessary to look for any other cause, or at least quite sufficient to render our investigations uncertain when so conducted. That is to say, I think the results will rather consist of determinations of the kind of electricity produced by friction of the hand upon the several substances, than any new or peculiar affection of the substances themselves.

Objection from the effect of friction by the hand.

Theory of electric excitation.

Among other interesting observations, for which I must refer the reader to the work itself, he gives a simple theory of the excitation of glass, and other electrics used in the construction of our machines. He remarks, that when the silk

flap is rubbed by the revolving glafs cylinder it is brought into clofe contact; and the electricity adhering more forcibly to the glafs, is carried forward into the open air, which air not having been rendered negative like the filk, does not counterbalance the furface of the glafs, and therefore its capacity being leffened, it emits the charge it had juft abforbed. And he thinks the amalgamated cufhion affifts the procefs, by bringing a furface of a conducting quality, and in connection with the earth into clofer contact.

Cavallo's experiments of electricity afforded by the fall of conducting bodies upon a tin plate.

The experiments of Mr. Cavallo were made by letting fubftances fall *out of his hand for the moft part* upon an infulated plate of tin, whence they were fhook off upon a table, or upon a chair; from which he again took the fubftance up, and let it fall upon the tin plate to the number of times required. After thefe reiterations he applied his tin plate to the plate of his multiplier. In his experiments a piece of zinc little more than half an ounce, was dropped ten times fucceffively upon the plate, which laft, as appeared by the fubfequent operations of the multiplier, was electrified negatively. Another piece dropped ten times produced the fame effect. Zinc was heated to 110 degrees, and one repetition of the experiment produced the fame effect, but ftronger. A fhilling, an half crown, a new guinea, a piece of copper, a piece of malleable platina, with like manipulation, produced fimilar effects, but differing in degree. Platina produced very little electricity; but when heated it was found to produce an effect contrary to the preceding; that is to fay, it electrified the tin plate pofitively. A piece of lead appeared to produce negative electricity in the tin; but when hot pofitive. A piece of iron afforded very equivocal refults. A piece of grain tin afforded negative electricity as well when heated as when cold.—When the cold tin was dropped from a pair of iron tongs, and let fall from the tin plate upon a chair, whence it was picked up by means of the fame iron tongs, it produced weak pofitive electricity. Tin when heated afforded electricity of the fame quality, but greater in quantity. Mr. Cavallo alternately ufed the hand and the iron tongs with refults generally like the preceding, that is to fay, negative electricity in the tin plate, when the grain tin was dropped from the hand, and pofitive when it fell from the tongs. Bifmuth produced pofitive electricity; but when the bifmuth was made very hot, the

Cavallo's experiments of electricity afforded by the fall of conducting bodies upon a tin plate.

the electricity of the tin plate was negative. These results were the same, when the bismuth was cast into a smooth flat piece, instead of being broken from a lump. When the iron tongs were used with the bismuth, the tin plate became negative, contrary to what it was when the hand was used.

An insulated silver spoon was substituted in the place of the tin plate, and in this zinc dropped from the hand produced negative electricity, which was stronger when the zinc was heated. The kind of electricity was not changed when the spoon was heated, and the zinc cold, but its degree was much less. Mr. Cavallo considers it as a very extraordinary fact, that the experiments varied as to the intensity of the electricity very much on different days, and is inclined to ascribe this difference to the disposition of the atmosphere.

In order to discover the source of the electricity produced in these experiments, he repeated them in a great variety of ways, namely, instead of the hand, he dropped the zinc from a tin plate held with one hand into a spoon, and from the latter back upon the former. He performed this operation with both the tin plate and the silver spoon insulated. He likewise tied a silk thread to the zinc, and holding the other extremity of the thread in one hand, struck the zinc repeatedly against the spoon; but in those cases very seldom any electricity was manifested, except when the weather, and every other circumstance was very favourable, and then the electricity could with difficulty be manifested; yet when the tin plate was held in the hand, and the zinc thrown from it into an insulated spoon, some electricity was more frequently produced than in the other two cases.

Whether animal electricity can be deduced from the preceding facts? &c.

After a careful review of his experiments, he doubts whether the phenomena of animal electricity can be attributed to the cause supposed to operate in them. For as he remarks, the action of metallic bodies produces the same effects, with hardly any observable difference, upon prepared animal limbs; whereas the effects in his experiments were fluctuating, and differed considerably in zinc and bismuth, which nevertheless do not excite the animal electricity more powerfully than zinc and silver, or zinc and gold. He also remarks, that he found by experiments with minute quantities of electricity applied to the prepared limbs, that they were not excited when these quantities were nevertheless very much greater than what was

produced

produced in his other experiments. But on the whole, he thinks they feem to eftablifh, 1. That the contact of one metallic fubftance with another generally produces electricity. 2. That the quantity and quality of the electricity fo produced, is various, according to many circumftances which feem to occur in the products of it, or in a great meafure to influence it. 3. And that thefe circumftances are, the various nature of the metallic fubftances, their various degrees of heat, the ftate of the atmofphere, the hand of the operator, &c. each of which caufes has its fhare in the refult.

VI.

On the Formation of Cryftals, defcribing a Method of producing them large and regular. By CIT. LE BLANC *.

It had long fince been remarked, that the fame falt is fufceptible of cryftallizing under feveral different forms. C. Hauy has demonftrated, that all thefe fecondary forms are owing to different arrangements of the fame integrant molecule; he has fhewn that thefe effects are not the effect of what is termed chance, but that they proceed from laws fufficiently fimple, which may eafily be determined. Here he has ftopped; he has not yet thought proper to publifh the ideas which he has given in his courfe [of lectures] relative to the caufes which difpofe the integrant molecules to follow fuch or fuch a law in their mutual arrangement. Thefe are the caufes which Cit. Le Blanc inveftigates in his obfervations on the growth of cryftals. He has been long engaged in thefe inquiries, and the memoir which he has read to the Inftitute is a confimation and fequel of that which he read to the Academy of Sciences, of which an extract was inferted in the Journal de Phyfique, November 1788, p. 374. He has difcovered, by his perfevering and ingenious obfervations, that we may confiderably vary both the bulk and form of the cryftals at pleafure, by caufing them to be formed and to grow under certain circumftances, and he has long fince en-

On the caufes of cryftallization.

Cryftals of extraordinary fize and beauty.

* Communicated to the French National Inftitute, and abridged in the Bulletin des Sciences, whence the above is tranflated, No. 50, An. 10.

riched the collections with cryftals of alum, fea-falt, fulphate of copper, &c. of an extraordinary fize and beauty: he now publifhes the means which he has employed.

How made.

Flat-bottomed veffels of glafs or porcelain, are the beft for obtaining beautiful fingle cryftals. The folutions ought to be brought to the point of cryftallization. They firft yield cryftals that are very fmall. Amongft thefe fmall cryftals, which Cit. Le Blanc calls embryos, a felection is made of the neateft, in order to promote their growth, or as C. Le Blanc terms it, to *nurfe (elever)* them. The liquor is decanted in order to purify it, and the fmall cryftals that have been fe-lected are diftributed in it, and carefully turned every day. Amongft thefe cryftals a fecond felection is made, in order feparately to nurfe thofe of which we wifh either to augment the volume, or change the form.

Embryo cryftals nurfed or reared, by felecting the placing them in the mother water.

In order to make them grow without irregularity, they muft be placed in the mother-water of a folution that has afforded a confufed cryftallization Care muft be taken to turn them often, and to give them frefh fupplies of mother-water in proportion as they increafe in growth. In this manner they may be brought to a confiderable volume.

If they be left too long in a folution in which they have acquired their full growth, they diminifh inftead of increafing in fize, and it is obferved that this decreafe takes place at the angles and edges, fo as to leave ftriæ vifible, which indicate the direction of the ranges of fubjacent molecules.—The pofition of the cryftals in the folution influences their form: this is particularly remarkable in the prifmatic cryftals: they grow in length when they are laid upon one of their fides, and in breadth when they are placed upon their bafe.

They may be made to grow either in length or breadth.

Secondary forms.

Cit. Le Blanc having changed octahedral alum into cubic alum, by placing an octahedral cryftal in a folution of alum faturated with its earth, which gives the cube, infers from thence that frequently the fecondary forms are owing to differences in the proportion of the principles *.

A curious

* It appears to us that this fact cannot lead, more than any other, to fuch an inference. According to the experiments of Cit. Vauquelin, the alumine in excefs is mixed with the fulphate of alumine, but it is not combined with it; for fimple folution in water is fuffi-cient

A curious obfervation of Cit. Le Blanc, which has already been recorded in the Journal de Phyfique, proves that the fame folution left to itfelf, is not equally faturated in all its parts. If we fufpend cryftals at different heights in a folution, the loweft cryftals increafe more rapidly than the higher ones; and it fometimes happens that thefe diffolve whilft the lower ftill continue to grow. Citizen Le Blanc adverts to the analogy that fubfifts between this obfervation, and that of the more complete faturation of the water of the fea at great depths.

Solutions are more faturated at greater depths.

Cit. Le Blanc informs us, that by adding fulphate of copper which cryftallizes in oblique prifms, to fulphate of copper which cryftallizes in the octahedral form, rhomboids are conftantly obtained *.

VII.

Accounts of the New Planet Ceres †.

On the 4th of February a letter from Dr. Mafkelyne was read before the Royal Society, announcing that he had obferved the new planet of Mr. Piazzi paffing the meridian between three and four o'clock in the morning, having about 188° 43′ right afcenfion, and 12° 38′ north declination, appearing like a ftar of the eighth magnitude.

Letters to the Royal Society, announcing obfervations of the new planet by Dr. Mafkelyne, Mr. von. Zach, and A. Aubert, Efq.

Another letter from Mr. von Zach was read, informing the Society that he had obferved this planet at Sceberg on the 7th of December, within half a degree of the place before

cient to feparate it. Thefe cryftals are accordingly opaque: befides, the fame chemift has obtained cubic and tranfparent cryftals from acidulous fulphate of alumine. (Note of the Editors).

* We muft obferve that the primitive form of the fulphate of iron is the rhomboid, and that the irregular octahedron which it prefents is a fecondary form. Cit. Hauy has examined one of thefe cryftals refulting from a mixture of a folution of fulphate of copper with a folution of fulphate of iron. The rhomboid which he examined differed in no refpect from the primitive rhomboid of the fulphate of iron. (Note of the Editors.)

† This whole article is taken verbatim from the Journal of the Royal Inftitution.

determined in his journal. Mr. Olbers ſaw it at Bremen on the 2nd of January. With a power of above 120 it preſented no obſervable diſc.

On the 11th a ſecond letter from the Aſtronomer Royal informed the Society that he had repeated his obſervation of the new planet, ſo as fully to aſcertain its motion. It appeared to have a viſible diſc when on the meridian, and viewed with a power of 50. When the air was very clear the diſc was round and well defined, but ſomewhat ſmaller than that of the 34th of Virgo, a ſtar of the 6th magnitude near it. Dr. Maſkelyne obſerves, that the ſmallneſs and roundneſs of the appearance of the diſc of the fixed ſtars is a good criterion of the clearneſs of the air.

Another letter from Alexander Aubert, Eſq. F. R. S. was alſo read. Mr. Aubert diſcovered the planet Ceres on Sunday morning, having about 188° 41 right aſcenſion, and 13° declination, its motion at preſent being retrograde.

Hiſtory of the planet as diſcovered by Piazzi; obſerved by Olbers, and conſidered by Oriani, Zach, Bode, Lalande, and Burckhardt.

Citizen Burckhardt in the Moniteur, 4 *Pluv. An.* 10. *No.* 124. gives the following account. The planet which Mr. Piazzi diſcovered at Palermo the firſt of January 1801, was again ſeen the firſt of January 1802, by Mr. Olbers, at Bremen, nearly in the place where it was expected from the calculations of Mr. von Zach. The 2nd January 1802, at 18^h. 58′ 36″, mean time, at Bremen, its right aſcenſion was 185° 9′, and its declination 11° 9′ north, in the wing of Virgo, near a ſtar of which Lalande had given the poſition, in the Connaiſſance des Tems, Year 9, p. 254. The 5th January, at 17^h. 36′, its right aſcenſion was 185° 43′, and its declination 11° 8′, nearly. It appears as a ſtar of the ninth magnitude, but it will become more conſpicuous. With a teleſcope magnifying 106 times, it cannot be diſtinguiſhed from a ſmall ſtar.

The 1ſt January it fortunately made a right angled triangle with two ſmall ſtars mentioned in Lalande's Hiſtoire Céleſte; the following day the form of the triangle was changed, and by means of this change the planet was recogniſed. It will be on the parallel of the 20th of Virgo.

The elements of this planet have occupied ſeveral aſtronomers. Meſſrs. Oriani, Zach, and Bode, had ſuſpected at once that it was a planet, becauſe it had been obſerved ſtationary, and without nebuloſity. But having received only two complete obſervations, they had not been able to confirm their

their fufpicions. Some time afterwards, Mr. Lalande firft obtained a complete copy of the obfervations of Mr. Piazzi, who could not refufe them to one, under whom he had fo long applied to the ftudy of aftronomy. By means of thefe obfervations, I was the firft that demonftrated, in a memoir prefented to the National Inftitute, that there was no parabolic orbit that could agree with the obfervations, although confined to an arc of 10 degrees. I gave at the fame time the elements of a circular and of an elliptic orbit, and I fhowed the great uncertainty that neceffarily remains when the elements are deduced from fo fmall an arc.

Having received a more exact copy of thefe obfervations, Mr. Olbers endeavoured to determine from them the elements of an elliptic orbit; but he found fo much uncertainty that he was obliged to prefer a circular orbit, fince he thought it impoffible to determine if the planet was near its aphelion, or its perihelion. I had proceeded on the former fuppofition; Mr. Gaufs preferred the latter, and endeavoured at the fame time to accommodate his calculations to all the obfervations of Mr. Piazzi: and this he performed with a difference of only a few feconds. Thefe are his elements: Elements of its orbit

Epoch of 1801 - -	2*s*	[17 ?]°	36′	34″
Aphelion - - -	10	26	27	38
Node - - - -	2	21	0	44
Inclination - - -		10	36	57
Greateft equation of the centre		9	27	41
Heliocentric and tropical diurnal motion			12	50.914

Mean diftance 2.7673. Eccentricity .0825.

Revolution 1681 days, or 4 years 7 months.

I had found the revolution 5 months and a half fhorter.

According to M. Lalande's calculations, Mr. Gaufs's elements give the longitude greater by a degree than Mr. Olbers's obfervation: according to Mr. von. Zach, my elements give it four degrees lefs, and Piazzi's, ten degrees lefs than the obfervation.

The idea of fearching for this planet among the immenfe collection of obfervations of the Hiftoire Célefte Françaife, could not fail to prefent itfelf to all thofe who have attended to the fubject: but it was impoffible to undertake the inquiry with any hopes of fuccefs, before the elements were corrected by new obfervations. I fhall now apply to it without delay. give the power of fearching for the planet in the collections of obfervations.

Mr. Piazzi has named his planet Ceres Ferdinandia. Lalande propofes to call it Piazzi.

Some account of M. Piazzi.

Mr. Piazzi was born at Ponte in the Valteline; and was profeffor at Malta, and at Palermo. When an obfervatory was about to be eftablifhed at Palermo, in 1787, he came to Paris; he then went to London, where he procured fome excellent inftruments: and he has already publifhed two volumes of valuable obfervations; he is now preparing to meafure a degree in Sicily, and Mr. Lalande has already fent him inftruments for this purpofe.

Extract from Bode's Kurzer Entwurf der Aftronomifchen Wiffenfchaften. Berlin. 1794. § 387.

Numerical analogy by which *Bode* fufpected the exiftence of a planet between Mars and Jupiter feven years ago.

Is it probable that Uranus, or the Georgian planet, is really fituated at the utmoft limit of our folar world? This appears to be very doubtful, confidering the immenfe fpace interpofed between it and the neareft fixed ftars. Other planets perhaps may be ftill more remotely fituated, and may perform their revolutions unfeen by human eyes. We can fcarcely fuppofe that any planet exifts nearer to the fun than Mercury: but confidering the proportions of the diftances of the planets from the fun, we obferve between Mars and Jupiter, a diftance far greater than a comparifon of the other diftances would lead us to expect, and this fpace may perhaps be occupied by a planet yet unknown.

This appears to follow from a certain proportion which we find among the diftances of the feven planets already known. Calling the diftance of Saturn 100, that of Mercury will be nearly 4, of Venus $4 + 3 = 7$; of the Earth $4 + 2 \times 3 = 10$; of Mars $4 + 4 \times 4 = 16$; we then want a planet at the diftance $4 + 8 \times 3 = 28$: the diftance of Jupiter is $4 + 16 \times 3 = 52$; of Saturn $4 + 32 \times 3 = 100$; and of the Georgian planet $4 + 64 \times 3 = 196$.

Obfervation; by whom?

It will, however, ftill be doubted by many if the conjecture quoted from Profeffor Bode can be thought to have been probable at the time that it was made. Calling the diftance of the Earth 10, the real proportional diftances are, in the neareft units, Mercury 4, Venus 7, Earth 10, Mars 15, (Ceres 28,) Jupiter 52, Saturn 95, Georgium 92. Inftead of Mars 16, Saturn 100, and Georgium 196.

Letters

Letters from Sir Henry Englefield, Bart. F. R. S. to Thomas Young, M. D. F. R. S. on the Planet Ceres.

SIR, Blackheath, *Friday.*

I have feen the new planet twice, on Sunday night, and again laft night. It is juft vifible to a common night-glafs. With a power of 90 in my great telefcope it was lefs bright than the 34 Virginis, near which it is. With a power of 200 no difc is vifible, and with 300 I can fcarcely fay that it has a fenfible diameter, more than what arifes from irradiation, for fmall ftars feen with fuch powers always appear dilated. Obfervations of the new planet by Sir H. Englefield, Bart.

I looked at the Georgian foon after the new planet, but clouds came on, and I did not try 300 on it laft night. With 200 the Georgian is, I am fure, the brighter, and it was a very much more vifible object in the night-glafs.

Sunday.

Laft night I again faw the new planet, and obferved it with a power of 400. With this great power it feemed to have an apparent magnitude, but was extremely fmall, faint, and ill defined. I then turned the telefcope to the Georgian (which as you know is very near), and the fuperiority in fize and brightnefs was very ftriking. The Georgian was not well defined, but I am fure it was full four times the diameter of the new planet, and much brighter in proportion to the different fize. Indeed the brightnefs of the Georgian is very furprifing, its vaft diftance from the fun being confidered. I really think that the diameter of the new planet cannot exceed a fecond; and it is of a very faint light even for that diameter. I looked then at the double ftar gamma Virginis, and faw the two ftars diftant from each other full three times their apparent diameter, a proof of the good adjuftment and high power of my telefcope. Its diameter very fmall.

I am, &c.

H. E.

VIII. *Obfervations*

VIII.

Obſervations and Experiments relating to the Pile of VOLTA. *In a Letter from* JOSEPH PRIESTLEY, *LL.D. F.R.S.*

To Mr. NICHOLSON.

DEAR SIR,

Galvanic pile.

HAVING been favoured by Mr. Weatherby Phipſon, a young man of Birmingham, with an excellent apparatus for repeating the experiments on the *pile of Volta,* conſiſting of ſixty plates of copper coated with ſilver, and as many thin rolled plates of zinc, which is a valuable improvement of his own, I have had great ſatisfaction in obſerving the reſults; and though, receiving intelligence of what is doing on the continent of Europe ſo late as I do here, it is probable that I ſhall be anticipated in my obſervations, I ſhall lay them before you, and, with your approbation, before your readers, after obſerving, that I have lately received the fourth volume of your excellent *Journal,* but am ignorant of all that has been done ſince the publication of it.

Its admirable effects not admitted to prove the decompoſition of water.

I cannot help expreſſing my admiration of the ingenuity with which your correſpondents and others have purſued this moſt curious ſubject; and in general my reſults are the ſame with theirs, though I draw different concluſions from them, eſpecially with reſpect to the modern hypotheſis of *the decompoſition of water,* which, though almoſt univerſally received at preſent, I conſider as wholly chimerical, and unable to ſtand its ground much longer. Indeed, I perceive that doubts are entertained concerning it by ſeveral of your correſpondents, and others obſerve that theſe experiments give no ſupport to it. To me it is evident that they are far from doing ſo. For though it may happen that the inflammable air from the wire connected with the ſilver end of the pile, be in the proportion to the dephlogiſticated air from the wire connected with the zinc end, which that hypotheſis requires, it appears that the latter comes from the air that is merely held in ſolution in the water in which the proceſs is made; ſince if, by means of oil upon the water, or a vacuum, acceſs to the atmoſphere be cut off, the whole production of air ceaſes. There is alſo no production of air when the water has been exhauſted of it; and certainly no good reaſon can be given why, if the water itſelf

For the oxigen does not bear the alledged proportion to the hidrogen;

but ariſes only from air diſſolved or abſorbed by the water.

The air ceaſes to iſſue, if the at-

itſelf conſiſts of theſe two kinds of air, and this proceſs be capable of decompoſing it, air ſhould not be produced from it in all theſe caſes; both the conſtituent parts of the water being preſent, and the power of ſeparating them being in full operation. Beſides, I find that though the two kinds of air be produced, they are not always in the proportion required by the new theory, the dephlogiſticated air being much leſs than is requiſite. I have alſo found it not much better than atmoſpherical air. The inflammable air I believe to be of the pureſt kind.

mosphere be excluded by oil or a vacuum.

Other facts.

If this inflammable air come from the decompoſition of the water, the water from which it is extracted ought to contain an overplus of oxigen, either in the form of dephlogiſticated air, or of acid. But the ſigns of acidity bear no proportion to the quantity of inflammable air produced, and can hardly be perceived at all. I did perceive it when I made the proceſs in water tinged with the juice of litmus, but only by the redneſs of the froth from the wire connected with the zinc end of the pile, the liquor itſelf remaining unchanged, notwithſtanding a copious production of inflammable air from the other wire. Alſo, when I introduced a piece of raw fleſh inſtead of the metal connected with the zinc end of the pile, no air came from it, nor did I perceive that the ſurface of it had acquired any acidity, though inflammable air was produced in great plenty from the other wire.

The ſuſpended metal *ſuppoſed to be ſuperſaturated with phlogiſton.*

But, except gold or platina be connected with the zinc end of the pile, there is ſeldom any production of air from that quarter, the metal in that ſituation being diſſolved; and there is no appearance of its being diſſolved by any *acid*, but, on the contrary, of its being ſuperſaturated with phlogiſton. But before I produce the evidence of this, which affords an argument againſt the decompoſition of water that appears to me to be perfectly deciſive, I ſhall relate ſome circumſtances concerning this ſolution of metals, which I do not find to have been noticed by others.

Metallic ſolutions by galvaniſm.

In general, wires connected with the zinc end of the pile are diſſolved, but none ſo readily as thoſe of ſilver, even when the wires connected with the ſilver end of the pile are of the ſame metal, and give air copiouſly; but if zinc or iron be connected with the ſilver end of the pile, any other metal, except gold or platina, connected with the zinc end will be

diſſolved.

diſſolved. Iron and zinc were the means of diſſolving each other. But the ſureſt method of producing this ſolution of metal was by connecting *charcoal* with the ſilver end of the pile. I once diſſolved pure gold in this manner, and I preſerve the ſolution as an evidence of it; but I could never do it a ſecond time, though I tried charcoal in ſeveral ſtates, perfect and imperfect, &c. nor could I by this proceſs diſſolve platina.

Gold.

Charcoal not diſſolved, &c.

Charcoal itſelf is not ſenſibly diſſolved in this proceſs, and air comes from the pieces connected with both ends of the pile. Suſpecting that this air might be that which always comes from charcoal when it is plunged in water, I filled the pores of two pieces of it with water, by means of the air pump, leaving them a long time *in vacuo*. Being then tried, they gave no air of ſome hours, but from the piece that was connected with the zinc end of the pile there proceeded a white cloud, which filled part of the veſſel of water. This, however, ſoon diſappeared, the water becoming tranſparent again; and after ſome hours both the pieces of charcoal gave air as copiouſly as any of the metals had done, and continued to do ſo as long.

Iron; zinc; copper.

When iron was connected with the ſilver end of the pile, and copper with the zinc end, the latter was diſſolved, but not till after two or three hours. Zinc being connected with the ſilver end, and copper with the zinc end, the former gave air copiouſly from the beginning, but it was near two hours before the copper began to diſſolve, which (being a flat piece) it did at the corners and edges only, and never from any part of the flat ſurface. When I added more copper, it began to give air without diſſolving, and alſo ſome of the green precipitate, which had been formed before, gave out air, and, the bubbles adhering to it, it roſe from the bottom of the veſſel to the top. This precipitate from the copper, which at firſt was green, became afterwards of a dark brown, as if the metal had been revived. This too was the caſe with one veſſel in which a ſolution of ſilver had continued ſome time. It has given a coating to the glaſs that is perfectly white and brilliant.

In four veſſels of water, with wires forming the circuit, the pheno-

Having introduced four veſſels of water between the two ends of the pile, and having connected each two with ſilver wires, that leg of the wire, in all the veſſels, which was next

to

to the ſilver end of the pile gave inflammable air, while, in all of them, the other leg of the ſame wire was diſſolved. When I covered one of theſe veſſels with oil, the production of air and the ſolution of the wires ceaſed in them all.

mena ceaſed, when one of them was covered with oil.

Though, as I have obſerved, there was a ſlight appearance of acidity in the water when dephlogiſticated air was given out at the wire communicating with the zinc end of the pile, there never was the ſmalleſt appearance of it when the metal was diſſolved. When ſilver was diſſolved in water tinged with the juice of litmus, and there was a copious production of inflammable air from the oppoſite wire, I could not perceive the leaſt change of colour in the water.

No acid found when ſilver was diſſolved.

I examined the water in which the proceſs was made, eſpecially when ſilver was diſſolved in it, but was ſo far from finding the air contained in it more pure than before, that it was evidently leſs ſo. Before the proceſs, the ſtandard of this air, with an equal quantity of nitrous air, was 1.1; with the water made turbid and white with the ſolution of ſilver, it was 1.2; and after ſtanding till it became black, it was 1.3.

The air in the water was worſe than common air.

The black matter from this ſolution of ſilver did not contain any oxigen, but was evidently the metal ſuperſaturated with phlogiſton; for when it was heated in dephlogiſticated air, it diminiſhed it, and converted part of it into phlogiſticated air; and when it was heated in inflammable air, it added to the quantity of it, and this appeared, by its exploſion with dephlogiſticated air, to be as pure as other inflammable air; ſo that this black powder of ſilver is ſimilar to the black powder of mercury made by agitation in water, which I have ſhewn to be mercury ſuperſaturated with phlogiſton. Where, then, is the oxigen that ought to be produced in great quantity, if the inflammable air from the wire connected with the ſilver end of the pile came from the decompoſition of the water?

The black powder of ſilver contains hidrogen, &c.

The glaſs veſſels in which ſilver has been diſſolved in theſe proportions are tinged of a dark colour, which no acid, nor any other menſtruum that I have applied, will take out. This is ſimilar to the caſe of flint glaſs becoming black by heating inflammable air in it, the calx of lead in the glaſs uniting with the phlogiſton of the air. In this caſe, therefore, it is natural to infer that this calx of ſilver imparts phlogiſton to the glaſs, and that there was nothing of oxigen in it. The ſurface of this black powder of ſilver long expoſed to the air while it is

The glaſs veſſels are blackened, as with hidrogen.

moiſt

moiſt becomes white, which is ſimilar to the experiment with mercury, in which the black powder of this metal, produced by the agitation of it in water, becomes white running mercury as it becomes dry, diminiſhing and phlogiſticating the air in which it is confined.

Experiment to ſhew that the black powder of ſilver, treated by nitrous acid, contains no oxigen.

The black calx of ſilver, made by its ſolution in nitrous acid, is ſaid by Mr. Macquer, in his Dictionary, to be owing to the phlogiſton contained in that acid. To aſcertain this, I diſſolved ſome pure ſilver in ſpirit of nitre, and evaporating the ſolution, I heated the reſiduum in thirteen ounce meaſures of dephlogiſticated air, of the ſtandard of 0.82, with two equal quantities of nitrous air, by which it was reduced to eleven ounce meaſures, of the ſtandard of 1.72, with one meaſure of nitrous air; ſo that it was almoſt wholly phlogiſticated air. Conſequently this calx of ſilver contains no oxigen.

Theory. That in the oxidation of the zinc there are plus and minus ſtates of phlogiſton produced in the apparatus, and that phlogiſton is electricity, or nearly ſo.

My preſent opinion concerning the theory of this curious proceſs is as follows. Since the operation wholly depends on the calcination of the zinc, which ſuffers a great diminution of weight, while the ſilver is little affected, and all metals loſe their phlogiſton in calcination, what remains of the zinc in a metallic form in the pile, and every thing connected with that end of it, is ſuperſaturated with phlogiſton, while the calcined part, and every thing connected with that end of the pile, is deprived of it. The former, therefore, is in a *poſitive* ſtate, and the latter in a *negative* one, with reſpect to phlogiſton; and it ſeems to follow from theſe experiments, that this is the ſame thing with poſitive and negative electricity; ſo that the electric fluid and phlogiſton are either the ſame, or have ſome near relation to each other. The ſilver ſeems to act principally as a conductor of electricity; for the ſurface of it is only blackened in ſome places in this proceſs, in conſequence probably of receiving phlogiſton from the zinc; but the water is moſt eſſential to it, becauſe it conſtitutes the principal part, if not the whole, of the addition of weight in the calx. Accordingly, in the calx of zinc I have found nothing but water, though it is probable that there is a ſmall portion of oxigen in it.

or two electric fluids, oxigen and phlogiſton.

Theſe experiments favour the hypotheſis of *two electric fluids*, the poſitive containing the principle of oxigen, and the negative that of phlogiſton. Theſe united to water ſeem to conſtitute the two oppoſite kinds of air, viz. dephlogiſticated and inflammable.

Theſe

These experiments tend likewise to confirm the conjecture which I advanced in my first publication on the subject of air, concerning *the similarity of the electric matter and phlogiston*; and, together with the proper galvanic experiments, shew, that the same substance elaborated from the aliment by the brain is the cause of muscular motion, the nerves being the most sensible of all electrometers. See the first edition of my Experiments on Air, vol. I. p. 274, &c.

Muscular motion.

I see no occasion to suppose, with Mr. Volta, that there is any circulation of the electric fluid in this pile. The calcination of the zinc supplies phlogiston as long as it continues, and when that ceases, the operation of the pile ceases with it. I also see no necessity that one end of the pile should be silver and the other zinc; and when both are silver, or both zinc, the operation is the same, nor can I conceive why it should be otherwise. When the pile is properly prepared, the addition of any kind of metal to the ends only serves as a conductor of the electric fluid; and silver, zinc, or any other metal, will sufficiently answer this purpose.

No circulation of electricity supposed.

Had this process succeeded without any atmospherical air incumbent upon the water in which it is made, it would have amounted to a full proof of the new theory, one part of the water being deprived of hidrogen, while oxigen abounded in the other, and both of them with the assistance of *caloric*, (though it does not appear whence that could be supplied), assuming the form of air. But this not being the case, the element of the dephlogisticated air evidently coming from the superincumbent atmosphere, the element of the inflammable air must necessarily come from the calcined metals which is a sufficient proof of the doctrine of phlogiston. Whether in this you will agree with me or not, I am,

Remark.

Dear Sir,

Yours sincerely,

J. PRIESTLEY.

Northumberland, in Pennsylvania,
Sept. 16, 1801.

P. S. In your *Journal*, vol. IV. p. 226. it is said, " the inventor of the galvanic pile discovered the conducting power of charcoal;" whereas it was one of my first observations in electricity,

electricity, made in 1766, and published in the first edition of my *History of Electricity* in the year following. See that edition, p. 598.

Experiment of the pile under a receiver, which completely deprived the air of oxigen.

Second P. S. After the above was written, I covered the whole pile with a large receiver, standing in water, charcoal being connected with the silver end of the pile, and silver with the zinc end, in two vessels of water; when the solution of the silver took place in both the vessels, and the air within the receiver began to diminish. The diminution having come to its maximum in about a day and an half, I examined the air within the receiver, and found it completely phlogisticated, being not at all affected by nitrous air. There was evidently, therefore, no dephlogisticated air generated, the whole result being the effect of the calcination of the zinc. This experiment, added to that on the black *calx of silver* produced in this process, and on the *water* in which it is made, completes the proof of there being no decomposition of water in this case, and strengthens the argument in favour of the doctrine of *phlogiston.*

IX.

Experiments and Observations towards determining the Influence of Oxigen on Germination. By Dr. CARRADORI *.

History of the subject. Authors: Boyle, Homberg, Muschenbrock, Boerhaave, Achard, Ingenhousz, Humboldt, Decandolle.

IT was long ago ascertained, by the experiments of Boyle and Homberg, that seeds require air for their germination, for it was proved that they do not germinate *in vacuo.* This necessity of air was afterwards confirmed by the experiments of Muschenbrock and Boerhaave, who had supported the observations of all times by the remark, namely, that seeds buried very deep under ground do not germinate, and that a great number of seeds do not even germinate under water, but perish in that situation. When this subject was afterwards studied with greater attention, in order to ascertain the reason why air is indispensible in this operation, it was found to be necessary in consequence of the portion of vital, or oxigen air which it contains: For Achard in the first place, and after

* Journal de Physique, LIII. Vendemiaire. 10.

him

him other philofophers, have fhewn that germination does not take place in any mephitic air; as, for example, inflammable or hidrogen air, phlogiftic or azotic air, which are known to contain no oxigen; whence it was neceffarily concluded, by a legitimate inference, that of all the atmofphere which we term the air, the oxigen is the only portion neceffary to germination. This has been confirmed in a more direct manner by Ingenhoufz, who difcovered, that the greater the abundance of oxigen the greater is the rapidity and facility of germination in grains or feeds; which probably led Humboldt to the difcovery of the means of accelerating germination, and effecting it even in the moft obftinate feeds, by the oxiginated muriatic acid, or even, as Decandolle pretends, of the nitric acid, which are known to be charged with oxigen.

Oxigen is thought to produce a change in the amylaceous part of feeds.

At prefent it remains to be afcertained what influence oxigen exerts upon growing feeds; that is to fay, what precifely are its effects upon germination. In order that germination may take place, it is neceffary that there fhould be a principle of fermentation in the farinaceous fubftance which refides in the cotyledons or placentæ of the feeds, which fubftance, as every one knows, envelopes and furrounds the germen or embryo; hence there is reafon to believe that this fubftance, combining with oxigen by means of fermentation, acquires a degree of acidity, and confequently a ftimulus proper for exciting the vitality of the germen, and for giving the firft impulfe to the circulation of the fluids in the embryo; or that, by means of the oxigen, this fubftance becomes modified fo as to acquire particular properties, by which it is rendered capable of affording the firft nourifhment to the tender fœtus or embryo of the plants; fince it has been demonftrated, by the late experiments of Dr. Rollo, that in the germination of the farinaceous feeds, the amylaceous part is converted, by means of the concurrence and combination of the oxigen, into faccharine fubftance, or even both.

Experiments.

It is not certain, however, that oxigen influences this only; that is to fay, that thefe are its fole effects: in order therefore to afcertain this point, I made the following experiments.

The feeds of wheat germinate moft fpeedily when the germ is in contact with oxigen.

I placed feeds or grains of wheat, *triticum*, in a plate with water, in the following manner: I fixed them to the bottom of the plate with foft wax, fo as to make them remain in a perpendicular or ftraight direction, fome with the germen upwards,

wards, others with the germen downwards, and fo that the water reached precifely to the level of their height; that is to fay, that it did not entirely cover them, in order that one of their extremities might be in contact with the air. The feafon was favourable to germination, it being the month of September, when the thermometer indicated 17° Reaum. or about 21° of the thermometer of 100 degrees. On examining them twenty-four hours afterwards, I found that thofe which had the germen upwards, or in contact with the air, germinated remarkably well, though all the reft of the fubftance of the feed was immerfed in the water; and that the others, which had been placed in the water with the germen downwards, though one of their extremities was equally in contact with the air, namely, the extremity oppofite to the germen, could not well germinate; they merely exhibited fome figns of germination; and there was no difference to be perceived between thofe grains and others which I had placed on their fide, not upright or perpendicular to the bottom of the plate, but lying down horizontally, and confequently covered entirely with water. Thus it appears that perfect germination does not take place unlefs when the part which contains the germen is in contact with the air, although all the reft of the feed be guarded againft its immediate contact.

Sprouted wheat grew well where the developed plant touched the oxigen; but did not when it was covered.

I took feeds or grains of wheat which had already germinated in the open air, and by means of the fame wax I placed them in the water, fome with the germen upwards, others with the germen downwards, in the fame veffel, and with the fame precautions, namely, not fuffering the water entirely to cover them; fo that thofe which had the developed germen, or the fmall plant directed upwards, were in contact with the air at this point only. When I examined them again, I found that thofe which had the germen downwards, and which confequently were not in contact with the air at this part, remained as they were, and exhibited no other fign of further germination; whilft in the others, whofe germen was expofed to the air, the germination advanced, and the fmall plant increafed remarkably in growth.

The ftalk is the part where the oxigen ought to apply.

I afterwards wifhed to afcertain, if it were indifferent whether all parts of the germen were in contact with the air, or only fome of its parts. For this purpofe, I chofe a number of grains of wheat which had germinated well for twenty-four hours

hours preceding, ſome of which I placed in the water with only the ſtalk projecting out of it, whilſt all the remaining part was immerſed under its ſurface, and others with the ſtalk under water, and the ſmall roots above its ſurface. I did not find that the germination made any progreſs with thoſe which had their ſtalk under water; but thoſe whoſe ſtalks were in contact with the air thrived and advanced in growth, and in a few days time the plant was developed.

Thus oxigen, or vital air, is neceſſary to germination, ſince the tender plant cannot thrive and grow without its immediate contact. Thus we ſee, that though it has received life by the effect of germination, it cannot preſerve it unleſs it be in contact with the air at this part; that is to ſay, at the ſtalk, which poſſeſſes an organiſation adapted to receive its benign influence, which is neceſſary to the ſupport of life. The germination commences without the immediate contact of oxigen, but it cannot become vegetation, if I may ſo expreſs myſelf, without its immediate contact. It ſeems, that in order to animate the germen or embryo of plants, and give it life, by means of the circulation and other functions, combined oxigen is ſufficient, but that it cannot afterwards ſubſiſt, if this element in a free and pure ſtate is wanting to the tender plant.

Germination may begin without free oxigen; but vegetation cannot proceed without it.

In fact, as I have already remarked, and as I have obſerved ſtill better in the ſubſequent experiments, the ſeeds begin to germinate under the water, but their germination ceaſes and proceeds no farther, unleſs the air receive the plant already born, or the animated germen. But germination is not effected under water unleſs in a certain caſe.

I ſelected a number of good ſeeds or grains of wheat; ſome of which I put into a plate, and poured in as much water as was preciſely neceſſary to cover them; others I put into a deeper veſſel, and covered them with a larger quantity of water; others I put into a glaſs, and covered them with a large quantity of water; and, laſtly, I put others into a bottle with a narrow neck, and covered them ſo that the water reached to the neck of the veſſel. After twenty-four hours, I found that the ſeeds in the firſt and ſecond veſſel had begun to germinate, but none of thoſe in either of the two laſt veſſels germinated, nor did they afterwards.

But no germination takes place unleſs the water be expoſed to the air, &c.

Hence it clearly appears, that as this difference can be referred only to the diverſity of the veſſels, that is to ſay, to the different ſurface which the veſſels in which the ſeeds had been placed expoſe to the air, and to the different quantities of water in which they had been immerſed; or, in other words, to the difference of the action or influence exerted by the oxigen contained in the atmoſphere upon the water: germination cannot even commence without the immediate contact of vital or oxigen air; provided, however, that the water in which the ſeeds are immerſed be in a condition to become changed with the oxigen, which is neceſſary to effectuate ſuch a function. It cannot be doubted but that water powerfully attracts a large quantity of oxigen from the air; it has a ſtrong avidity for this principle, and becomes abundantly loaded with it. M. de Gleichen and Senebier had obſerved that the ſeeds of peaſe, when placed in too large a quantity of water, periſhed without germinating, but they had given no accurate explanation of the fact.

under circumſtances favourable to its abſorption of oxigen.

Recapitulation.

I have thus eſtabliſhed two eſſential facts relative to germination, which, to the beſt of my knowledge, had not before been pointed out. Vital or oxigen air is neceſſary to the grand proceſs of germination; but in order to give the impulſe or the principle of this germination the immediate contact of the air is not neceſſary, but it is indiſpenſable to its continuation or progreſs; ſince the germen already animated, or the ſmall plant, cannot grow nor vegetate, unleſs it be in a ſtate to enjoy the immediate influence of this vital fluid. Ingenhouſz had only aſſerted in general terms, that the contact of oxigen is neceſſary to the germination of grains or ſeeds; but ſomething more particular was ſtill wanting.

Repetition of experiments, with other plants.

I have repeated the ſame experiments upon the ſeeds of barley, *hordeum vulgare*; of beans, *vicia faba*; lupines, *lupinus albus*; and have obtained the ſame reſults: hence we may conclude that the above-mentioned inferences are applicable to the ſeeds of all *terreſtrial plants*, that is to ſay, ſuch as ſow themſelves upon the dry ground.

Seeds of aquatic plants germinate and grow by the combined oxigen of water.

As to the ſeeds of aquatic or marſh plants, it appears, that, both for the commencement and for the continuation of germination, the oxigen which is combined with the water is ſufficient, without the concurrence of free oxigen, as we ſee that they germinate, and afterwards grow very well, under the

the water. Their organiſation, different from that of the terreſtrial plants, enables them to avail themſelves, for the ſame operations, of the oxigen which is diſſolved in the water.

Though ſeeds can germinate under water, yet by being kept there they loſe much of this faculty, and at length become altered to ſuch a degree as to be no longer capable of germination, even though we change their place, and tranſport them into dry earth. **Seeds are injured by lying in water.**

I have obſerved that ſeeds of wheat, that had been kept in water during three ſucceſſive days, but not under circumſtances to enable them to germinate, and were afterwards tranſported into dry ground proper for producing this effect, germinated almoſt with the ſame vigour as if they had been placed there at firſt; but the fifth day the greater part ceaſed to germinate, and thoſe which ſtill germinated did it in a languid manner, and the ſmall plants which they produced were ſickly, and without vigour. **Experimental proof,**

I have remarked, that the ſame happens with plants which have germinated under water; by remaining there during too great a length of time, theſe tender plants are ſpoiled, and no longer capable of growing and thriving, even though they change their ſituation, and are placed under more favourable circumſtances. I have alſo obſerved, that this too long abode in the water is the more dangerous to them when theſe plants are tender; that is to ſay, nearer to the period of their birth, or commencement of germination. **even after germination.**

Hence we ſee what danger ariſes from too long continued and abundant rains after the ſeed time. The too great quantity of water keeping the ſeeds for too great a length of time in a ſtate of ſubmerſion, prevents their germinating, or injures their healthy germination, and is the cauſe why the plants which they produce appear with a conſtitution more or leſs altered, in conſequence of the prevention of their communication with the air, or, in other words, for want of oxigen. Hence the wet winters, which, for the reaſons that have been mentioned, may prove dangerous to the ſeeds of wheat, the moſt important of the fruits of the earth, are juſtly conſidered as preſaging a ſcanty harveſt. **Hence the miſchief of abundant rains after ſeed time.**

——Hyemes optate ſerenas
Agricolæ: hyberno lætiſſima pulvere farra
Lætus ager—— VIRG. GEORG.

Dry windy winters are beſt for the ſeed.

Dry windy weather is then advantageous, ſince the oxigen neceſſary for the grand work of the germination and growth of the tender plants is furniſhed eaſily and abundantly by the atmoſphere. After this period, that is to ſay, after their infancy, the plants no longer require oxigen, in order to proſper, but a mephitic air, as I have already ſhewn in my memoir *On the Fertility of the Earth*,* becauſe in this they find their principal nouriſhment: thus when the period has elapſed, oxigen is but of very limited utility to vegetation.

X.

On the Choice and Uſe of a Razor. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

January 26, 1802.

IF you think the following obſervations applicable to your paper on Shaving, you will inſert them in ſome future Number of your Journal.

W. B.

On the choice of a razor.

In chooſing a razor (beſides the marks you enumerate) prefer that blade whoſe edge is leaſt blunted or turned after being two or three times drawn upon its edge, from heel to point, on a bit of horn or the thumb nail.

Straping.

In ſtraping care ſhould be taken to give the finiſhing ſtrokes in the direction in which it is about to be uſed: that is, if the drawing ſtroke in ſhaving is made from heel to point, the razor ſhould be drawn from heel to point upon the ſtrap, and *vice verſâ*. This obſervation applies to all fine edges, eſpecially to ſurgical inſtruments. For a ſtrong beard, a rounder edge is given by a looſe ſtrap; but a keener and more durable one is given by a ſtrap fixed upon ſome inelaſtic ſubſtance.

* Della Fertilita della Terra. Memoria del Doct. J. Carradori, Premiata della R. Societa Economica di Firenze nel meſe di Giugno del 1799.

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The ſoap lather, beſides the effects already mentioned, ſeems to act by giving a degree of firmneſs to the beard. It unites, in ſome meaſure, a great number of hairs, and thereby occaſions a more equal reſiſtance to the ſtroke of the razor, diminiſhing the elaſticity of the hair; and alſo taking off a little of its natural fine poliſh. In ſhaving without ſoap the beard is generally irregularly cut, eſpecially if the edge be not perfectly keen; as may be obſerved in cutting a bundle of briſtles looſely tied, or taking the edge off a quire of paper without putting it into a preſs. Provided the lather be of ſufficient conſiſtence, it is not material whether hot or cold; for I ſuſpect the alkali acts pretty quickly upon the poliſhed ſurface of the hair. The lather.

The form of the razor is of ſome conſequence. Young ſhavers, who may not be equal to giving the drawing ſtroke, ſhould chooſe a ſcimitar-like blade. Every razor ought to have the point terminated by a ſegment of the circle; otherwiſe it is difficult to give the ſtroke from the point towards the heel, which is often neceſſary when a man is not *ambo dexter*. The form of the blade.

Another Correſpondent has favoured me with what ſeems to be the moſt probable explanation of the effect of heat on edge tools. He obſerves, that in the cold regions of North America an axe will ſometimes fly in pieces like glaſs, and that our ſmiths in this country are well aware of the increaſe of tenacity which a very ſlight increaſe of temperature gives to ſteel, iron, and other metals, and take care to uſe it when they ſet or alter the figure of any tool or utenſil by cold hammering. They warm the article to prevent its breaking. He thinks the fine edge of a razor, which would ſplinter and become rough if ſtrapped or uſed cold, may perform its office much better when rendered more tenacious by a moderate heat. Effect of heat on edge tools.

W. N.

XI.

Obſervations on the Method of painting with Milk. By CITIZEN DARCET, *Member of the Lyceum of Arts, and Eſſayer of Money.* *

Importance of ſubſtituting caſeous matter for ſize in painting.

UPON reading the different articles of the Decade Philoſophique, in which the method of painting in milk is mentioned, and on examining the theory which Cadet-de-Vaux has developed reſpecting this uſeful application of our chemical knowledge, I perceived how important it would be if we could ſubſtitute the caſeous or cheeſy matter, which we poſſeſs in ſuch great quantities, to glue or ſize, which is commonly uſed in painting in diſtemper, and by this means to appropriate that ſubſtance to paper-hangings and other arts, for which it is an article of the firſt neceſſity. I apprehended that the proceſs of this new method of painting was ſuſceptible of being rendered more ſimple, and I endeavoured to ſtudy the different phenomena in order to aſcertain what ſubſtance might be left out in part, or even wholly, without altering the goodneſs of the colour. This examination has enabled me to make the following obſervations, which may ſerve as a ſupplement to the memoir publiſhed on that ſubject. †

Hiſtorical remarks.

I ſhall not attempt to ſolve the queſtion, whether painting in milk was known to the ancients. The ſolution of this problem, which is perhaps impracticable, is ſcarcely intereſting to us. I ſhall only remark, that the Indians, who uſed milk to dilute the colours with which they painted the ſides of their cabins, appear to have given the firſt notion of the application of a natural mixture of the caſeous part along with the ſerous part, to render colouring matter adheſive. ‡

For this application we are indebted to Cadet-de-Vaux, who has by theſe means afforded a real ſervice to the public,

* Decade Philoſ. No. 5, An. X.

† See Philoſ. Journal, quarto, V. page 247.

‡ Our houſe painters are alſo acquainted with the advantageous uſes of milk in inſide paintings. They have long uſed a mixture of milk and well waſhed lime to give a brighter white to delicate objects in relief, in order that they might appear more prominent, from the ceiling painted with the ordinary white-waſh.---D.

by

by rendering a material ufeful which in many places had, properly fpeakiug, little value, unlefs in the ordinary ufe of extracting the butyraceous part, which alone reprefented the whole value of the milk before its decompofition. He has at the fame time given a degree of perfection to this paint, by rendering the procefs fufficiently fimple, to afford a folid colour, nearly without fmell and at a moderate price.

Improved procefs for painting.

Thefe qualities, which painting in milk really poffeffes, appear to render it little fufceptible of amelioration. I fhall neverthelefs venture to propofe one which has fucceeded in my hands, and of which the experiments have been made on a fcale fufficiently extenfive to infure the goodnefs and the value of the refults I have obtained. *

After having given an account of the obfervations which led me to this procefs, I fhall fpeak of the materials I ufed, their proportional quantities, the beft method of combining them, and, in a word, the procefs to which we muft juftly apply the denomination of cheefe painting. †

Cadet-de-Vaux's two proceffes do not differ. Critique on his method.

Citizen Cadet-de-Vaux, in his memoir on milk painting, has given two proceffes, which, as it appears to me, ought to be confidered as one, becaufe the firft which he announces as fimilar to common diftemper does not differ from the fecond, which he gives as proper to be fubftituted inftead of oil painting, except in the Burgundy pitch, which conftitutes part of this laft. In fact, Burgundy pitch renders the colour more folid, but it is far from rendering it capable of being wafhed like oil paint; a property which is known to be truly charafteriftic of this paint, and is of itfelf fufficient to diftinguifh it from diftemper. I fhall not, therefore, examine thefe two proceffes feparately; and I fhall conclude my ob-

* I painted an entire chamber with the colour I fhall proceed to defcribe. Individuals, who flept in it the very day of the operation, were in no refpect incommoded by the fmell of the paint, though the door and the windows were fhut the whole night.

† We find, in the *Dictionnaire de Peinture de Pernety*, a note in which he fpeaks of a pamphlet entitled, *La Peinture au Fromage ou au Ramekin.* It was written in oppofition to the encauftic method of painting, of which Citizen Bachelier has, as it were, re-invented the procefs. I have not been able to procure this pamphlet, for which I am very forry, as it might probably contain fome data refpecting the method of painting on which we at prefent treat in a more ferious manner.---D.

jeƈt

ject to be attained, if the procefs I offer fhall afford a colour equally good as that which is obtained by following the procefs for the refinous milk paint.

We read at page 250 of the memoir to which I allude, that the oil added to the mixture of flaked lime and fkimmed milk is diffolved by the lime, and then forms a calcareous foap.

Lime, milk, and oil form a triple compound.

By carefully examining what paffes in this operation, I have thought I obferved that the lime does not feparate from the cafeous part to combine with the oil; but that the oil which is added in the mixture forms a triple combination, little foluble in fact, but perfectly diffufible in water. We know, on the contrary, that calcareous foap is infoluble and quite immifcible in water; and I have found that the addition of fkimmed milk produces no change in thefe properties. I have alfo remarked, that the triple combination does not take place, but in the order defcribed by Cadet-de-Vaux, for the lime does not entirely combine with a mixture of oil and fkimmed milk. In this cafe there is only a formation of calcareous foap, which remains in maffes fufpended at the furface of the liquid.

Sour milk unfit for the pigment.

Cadet-de-Vaux alfo announces, in page 248 of his memoir, that four milk is no longer proper for the compofition of this colour. He obferves, that the ferous part of the milk being converted by fermentation to the ftate of acetous acid, may then form a kind of calcareous acetite, which, by its deliquefcence, muft contribute to deftroy the colour in which it is mixed.

The whey is of little utility in the paint; but very ufeful in many other applications.

I have verified this fact, and I think that the ferous part cannot be ufeful in the compofition of paint, unlefs before that period in which the acefcence fhall have converted the fugar of milk it contains into acetous acid. For in the former cafe it is capable of giving folidity and brightnefs. But when we confider that the fermentation which produces this phenomenon takes place with much rapidity; that in many countries this ferous part is ufed in bleaching, in the preparation of fugar of milk, and for the feeding of animals, we fhall be defirous of retrenching it from a procefs in which it is fometimes ufeful, but oftener noxious, in order to appropriate it entirely to thefe feveral ufes.

By

By examining, in the fame manner, the effect of the addition of oil upon the reft of the mixture, we foon perceive that if it does not improve the folidity of the colour, it muft either be ufelefs or pernicious; becaufe it renders the tint dull, and communicates a difagreeable fmell. Now experiment demonftrates the negative, for milk paint in diftemper does not refift water more than the fimple mixture of fkimmed milk with flaked lime and whiting. The oil is of no value as an ingredient.

I have alfo concluded that the quantity of fix ounces, or 183,430 grains of flaked lime, prefcribed in the recipe for painting in diftemper with milk, was much too confiderable, becaufe two ounces, or 61,143 grammes, or even a fmaller quantity, is fufficient entirely to liquify one French pint, or or 951,206 cubic centimeters of fkimmed milk. The pellicle of calcareous carbonate, which is formed after a time at the furface of painting in milk diftemper, fhews, as well as other experiments on this fubject, that the prefent obfervations are well founded. The proportion of lime is too great.

With regard to the whiting, I think the dofe may be increafed without inconvenience. Cadet-de-Vaux already added three pounds, or 1467,438 grammes, to five pounds, or 2445,75 grammes. I have gone further, and have ufed a dofe of eight pounds, or 3913,168 grammes, and the colour I obtained did not appear to me lefs folid than that made according to the original recipe. This obfervation does not apply to the preparation, when required to be coloured either by an ocre, an earth, or an oxide. I have remarked, that thefe fubftances require more of the mordant than the whiting does; and this happens, in fact, with painting in diftemper with milk coloured yellow or red, in order to be applied to pannels, &c. namely, that the addition of thefe colouring matters deprive it of all its folidity. We muft not, therefore, add the colour to the paint entirely mixed, as is directed at page 248 of the memoir, but diminifh the dofe of whiting in proportion to the quantity of colouring matter neceffary to produce the required tint; in a word, it is requifite that the colouring matters added to the whiting fhould not weigh at moft more than the five pounds, or 244,573 grammes prefcribed in the recipe. The whiting may be increafed. Colour muft be added during the firft mixture.

Thefe are the principal obfervations I have made on painting in milk; let us now fee what are the confequence we may juftly derive from them.

I have

An improved compofit'on.

I have concluded that the oil or Burgundy pitch, the ferous part, and a portion of flaked lime, might be entirely left out of the recipe. My paint will, therefore, contain only the cafeous part, a portion of flaked lime and whiting. Reafoning pointed out thefe conclufions; but my doubts on the fubject were not yet cleared up by experiment.

I formed a great number of mixtures by conftantly varying in each of them, in oppofite directions, the dofes of cheefe, of lime and of whiting. Many of my experiments were ufelefs; but I fucceeded in difcovering the following procefs, which appears to me to anfwer the intended purpofe. The following are the dofes:

Proportion of ingredients.

Cheefe or curd well drained	144 grammes	=	5 oz. avoir.
Slaked lime - -	7 grammes	=	$\frac{1}{4}$ oz.
Whiting - - -	280 grammes	=	10 oz.
Fine powdered charcoal	2 grammes	=	1 dram.
Water - - -	80 grammes	=	3 oz.

The kind of cheefe.

This cheefe is commonly called *fromage à la pie,* or foft cheefe. I have ufed old cheefe which was almoft dry, which neverthelefs afforded good refults, but the frefh cheefe is certainly to be preferred. I muft alfo obferve that thefe cheefes differ from each other; they are not all equally proper for the purpofe, and I have found fome which compofe colours of little folidity; they were, in general, difagreeable to the tafte.

Manipulation.

Let us now fpeak of the manipulation. At the moment of commencing the operation, a certain quantity of ftrong quick lime muft be flaked in the leaft poffible quantity of water. This is the fureft and moft fpeedy method of reducing it into fine powder. The lime is to be fifted, in order to feparate the pieces which do not fall down, and of the powder feven grammes are to be weighed. The quantity of cheefe above indicated is to be taken and pounded till it has the appearance of falve, and with this the feven grammes of lime are to be mixed, and the mixture well agitated, which lofes its confiftence, and acquires that of hot new made glue.

On the other hand, whiting in powder is taken, and added to the water and the charcoal, and the whole accurately mixed. This mixture may be paffed through an open fieve, in order that it may be reduced to a liquid homogeneous pafte.

The

The mixture of lime and cheefe is then to be added, and carefully mixed with that of the whiting and charcoal diffufed in water. The colour is then finifhed.

The dofes here pointed afford a colour too thick to be ufed in this ftate. We muft therefore add to the mixture a quantity of water neceffary to communicate the degree of fluidity defired; but this addition muft not be made till the moment before it is ufed, for I have obferved that the colour keeps better the lefs water it contains. Dilution.

Two hundred and ten grammes of water added to the colour, made as before prefcribed, afford the neceffary quantity for exactly covering a fquare furface of 1,948 meters, or one toife or fathom. It may, however, be eafily apprehended that the dofes of water, and even thofe of charcoal, may be varied to a certain point, according to the judgment of the operator. Surface covered.

When a red or yellow colour is defired, fimilar to that which is ufed for pannels, ceilings, &c. I fubftitute, inftead of whiting and charcoal in the foregoing procefs, the colouring matter which I intend to ufe. The following are the preparations which appeared to me to afford the moft folid colour moft capable of fupporting the encauftic and wax *(l'encauftique et le cirage.)* Colour.

Well dried curd or cheefe -	144 grammes
Slaked lime - - -	7 grammes
Colouring matter - - -	200 grammes

If, inftead of the ocres, we fubftitute charcoal in a ftate of high divifion, lamp black, for example, we obtained a black colour, which may be ufed with fuccefs to blacken the leather of trunks. Shining effect by wax or fugar.

When the colour is dry, if it be defired to give it a fhining appearance, it is covered with two coats of a folution of white wax in the effential oil of turpentine. When this kind of encauftic is dry, the wax may be polifhed by friction with a clean cloth. This preparation has the advantage that it does not fcale, but refifts water a little, properties which the compofitions ufually applied to this purpofe do not poffefs.

If, to the mixture of cheefe, lime and lamp black, a fmall quantity of dried fugar or honey be added, a black colour is obtained, which dries fpeedily, and is fufficiently fhining to

3 be

be ufed like the foregoing. The experiments I have made on this object, though fatisfactory, are not yet perfectly equal to my wifhes.

The paint is mixed according to the preceding inftructions, and the neceffary degree of fluidity for painting a firft or fecond coat is given by means of water, the dofe of which muft be determined by circumftances and judgment. The whole is then covered with the common encauftic, and the operation is finifhed as ufual by rubbing.

Advantages of this new paint.

I fhall not enlarge more amply on the utility of the paint which I here propofe. Every obfervation which has been made by Cadet-de-Vaux may be applied to this, becaufe the various changes I have made in this procefs do not deprive it of any of its advantages; but, on the contrary, render it more valuable. The folidity of the painting by means of cheefe is at leaft equal to that of painting with milk and refin; but the former is more beautiful, lefs difpofed to become yellow, is more fimple, and muft neceffarily coft much lefs than the paint in diftemper with milk, and ftill much lefs than the refinous paint with milk. Water does not fpot it, nor even leave any trace after it is dried, an advantage which fize painting is very far from poffeffing.

It may be kept very well, or made into cakes.

This paint with cheefe may be kept very well, particularly when it does not contain much water. I have feveral times formed it into cakes, which, when pounded after their entire deficcation, formed, by the addition of a very fmall quantity of cheefe and lime, a colour as folid as that which had been newly made. This experiment fhews how much the export of this colour manufactured in agricultural diftricts may become advantageous in the way of trade.

How far this or any other paint can be a prefervative againft infection.

With regard to the property which the refinous paint with milk poffeffes of depriving walls of infection by its application upon furfaces penetrated with putrid exhalations; as this effect is purely mechanical, we may by analogy infer that painting with cheefe muft anfwer the fame purpofe, becaufe it is fufficiently adhefive and fluid completely to ftop the pores of the ftones, plaifter, and wood, to which it adheres no lefs ftrongly than the refinous paint with milk. But this property appears to me of little real utility. The proceffes which Citizen Guiton has given in his work on difinfecting the

the air, may be applied to the cafe mentioned by Cadet-de-Vaux, and anfwer the purpofe with infinitely more fpeed and perfection. It appears, therefore, that thofe methods only ought to be ufed in fuch cafes. For the fumigations operate of themfelves, and without the affiftance of a workman; whereas the painter of an apartment or infected chamber muft neceffarily expofe his health, and fometimes even his life.

It only remains, after having thus endeavoured to fimplify the procefs which Cadet-de-Vaux firft brought to perfection, that I fhould exprefs my wifh that the advantages it really poffeffes might engage the manufacturers of paper hangings to adopt its ufe. The inhabitants of the country, who muft naturally be more ftrongly impreffed with its advantages than thofe of towns, will, no doubt, be the firft to avail themfelves of it. This application may at leaft affift us in faving an annual tribute, which our manufacturers pay to foreigners for animal glue or fize. If cheefe could hereafter be ufed as a fubftitute in block printing, I fhould think myfelf happy to have contributed one ftep towards the perfection of an art no lefs ufeful than agreeable.

Suggeftion that it may be ufed in block printing of paper hangings.

XII.

Defcription of an Hydraulic Bellows for a Smith's Forge. By Mr. J. C. HORNBLOWER.

To Mr. NICHOLSON.

SIR,

I SEND you a fketch of a fmith's bellows a little out of the common way. Its application to my purpofe was fuggefted by an acquaintance I had by the large machines of that fort erected at Willy Furnace, by the late Mr. Ifaac Wilkinfon, father to the prefent Mr. John Wilkinfon, about forty years fince, and where circumftances or inclination have taken the lead, I have adopted the principle occafionally in this form.

Small hydraulic bellows for a forge.

What I have principally in view is, to mention to you a very ftriking difference between the effect of this bellows, and a common leathered 30 inch bellows in the fame fhop. The leathered bellows throws confiderably more air to the fire, and

Its effect compared with common leathered bellows.

its nozzle compared with this, is as ,73 to ,60 in diameter, but it does not produce ſo great an effect in bringing on the heat, and the voice of this is ſo great as almoſt to drown that of the common one. The only difference in other reſpects is, that in the hydraulic bellows the pipe goes underground for about eight feet, and the conducting pipe of the other comes down about the ſame diſtance from the ſhop above.

If any further particulars are neceſſary for your comment on this ſubject, they are at your ſervice,

I am, SIR,

Your very obedient Servant,

J. C. HORNBLOWER.

Eaſt Row, City Road, Feb. 12, 1802.

DESCRIPTION. Plate XII.

Deſcription of its ſtructure and operation.

A, The plunger, or working part of the bellows, 18 inches ſquare within, which receives the air by a valve in the hinder part opening inwards, which at the ſtroke by the rockſtaff E, throws it down the tube indicated by the dotted lines, which has a valve opening into the reſervoir D, whence it is led to the tuyere by the pipe P. Length of the plunger 20 inches, ſtroke nine inches. Diameter of P three inches; of the nozzle 0,6 inches.

The whole is placed in a pit or ciſtern, having water ſufficient to riſe to the lower end of the tube where the valve hangs; this tube is the only communication between the upper part and the reſervoir D: when as much water is poured in round the working part over the waſh boards, as will riſe within five inches of the upper edge of them, the bellows is ready for uſe. The little frame work ſerves to keep it from riſing, and affords a convenient ſupport for the balance and the rock ſtaff.

I ſhould obſerve, that the area of the pit or ciſtern ought to be at leaſt twice as much as that of the plunger A *.

* From the preſſure of other buſineſs, it has not been in my power to viſit this manufactory, in order to aſcertain the cauſe of the difference here ſtated; but I ſhall give the facts more particularly in the next number. W. N.

XIII. *On*

XIII.

On the Practicability and Advantages of a general System of Rail Roads, and the Means of carrying the same into Effect. In a Letter from RICHARD LOVELL EDGEWORTH, *Esq.*

To Mr. NICHOLSON.

SIR,

MANY years ago I formed the project of laying iron rail-ways for baggage waggons on the great roads of England; but having consulted several of my friends, who were eminent mechanics, so many objections were started, that I for some time despaired of success. One great objection arose from the vast expence of massive rail-ways, and the continual cost of repairs. To obviate this difficulty, it occurred to me to divide the weight that is usually carried upon a single waggon into four or five portions, and to place them upon four or five small carriages; these carriages linked together would be as easily drawn as the same load upon one waggon. In pursuance of this idea, about the year 1768 I presented models of three such carriages to the Society for the Encouragement of Arts and Manufactures, who for this and other inventions in mechanics, honoured me with their gold medal; the date of which, and the journal of the Society may ascertain the early claim which I have to this invention. In 1788 I constructed four carriages with cast iron wheels, truly turned and supported upon friction rollers; these were shewn to several eminent persons, and were employed upon a temporary moveable wooden rail-way for a considerable time, in carrying lime-stone for the improvement of land. A variety of accurate experiments, and some useful improvements in this mode of carriage were made with these machines, which it would take up too much of your valuable work to detail. I shall mention only one idea, which appears to me so practicable, that I beg a place for it in your Journal, which will secure to it the most extensive circulation here, and on the Continent.

The public advantage of rail roads long ago stated by the author,

with models, &c. to the Society of Arts; viz. in 1768.

Experiment in 1788.

I propose, that by way of experiment iron rail-ways should be laid on one of the great roads, to the distance of ten or twelve miles from the metropolis, upon something like the following

Plan for the commencement of a public rail-way. Four roads; two for

heavy carriages, out and home, and two for light.

lowing plan: four rail-ways ſhould be laid on the road, raiſed on ſleepers of ſtone, ſo that their upper ſurface ſhould ſtand about four inches above the road. They ſhould be made hollow from the bottom upwards, for ſtrength and to ſave expence broad at bottom, and rounded at the top, to prevent the lodgment of dirt and duſt. On theſe ſhould run light waggons, each containing not more than one tun and a half weight.

I have mentioned four rail-ways. The two inſide roads ſhould be appropriated for waggons, and the two external rail-ways for coaches and chaiſes, &c. The left hand rail-way invariably to be followed by each ſpecies of carriage on its own road; ſo as to prevent the poſſibility of any carriages meeting on the ſame rail-way. By appropriating the exterior tracks to light carriages, thoſe which wiſhed to paſs others might turn off upon the waggon road, and reſume their proper place after they had gone by the carriage they wiſhed to paſs. Now to accommodate coaches and chaiſes, &c. to theſe rail-ways, I would have them carried, wheels and all, in cradles or platforms; *which ſhould have wheels adapted to the rail-ways.* By theſe means no alteration would be neceſſary in any of the carriages commonly uſed; but the horſes of any coach or chaiſe might, as ſoon as they had got out of town, walk up an inclined plane into the cradle or platform, and draw their reſpective carriages after them: the horſes ſhould then walk out at the fartheſt end of the platform, upon the road belonging to the rail-ways. They would then draw the chaiſe not upon its own wheels, but upon the wheels of the platform or cradle in which the *chaiſe* ſhould be detained.

The carriages to be drawn ſtanding on platforms or cradles adapted to the rail.

Expected rate of travelling, 12 paſſengers, ſix miles an hour by one horſe, &c.

For ſtage-coaches ſimilar platforms ſhould be provided, and in theſe ſix inſide and ſix outſide paſſengers might travel at the rate of ſix miles an hour with one horſe. Hackney, or gentlemen's chaiſes might go at the rate of eight miles an hour with one horſe, without interruption or delay.

Hills avoided.

Where hills intervene, new roads muſt be made following the courſe of ſtreams that wind between the hills, a moderate acclivity would not obſtruct the progreſs of theſe carriages, that is to ſay, a riſe of one foot in ten.

Numerous benefits from this ſcheme.

Every perſon converſant in theſe ſubjects, muſt know how much within bounds I ſpeak with reſpect to the eaſe of draught upon rail-ways. The ſaving of horſes and their food, the ſaving of wear and tear of carriages, the increaſed diſtance to which

which horſes could travel in a day, the freedom from dirt and duſt, the ſecurity by night, the eaſe with which the ſick and infirm might be tranſported from place to place, are all obvious conſiderations; but the chief convenience of this project ariſes from the mode of receiving and tranſporting on rail-ways every carriage now in uſe *without any change in their ſtructure,* ſo that a traveller may quit and reſume the common road at pleaſure. To enumerate all the advantages that would ariſe from ſuch roads, is unneceſſary in this ſlight ſketch; the intelligent reader will probably perceive many that are not even hinted. It is ſelf-ſufficient to lay the general idea before the public; the contrivance and economy of the different parts of ſuch a project require long and minute details, which muſt be reſerved for another ſtage of the buſineſs. If ſuch a plan ſhould have a fair trial, it might lead the way to farther ſpeculations.

In this plan the carriages require no alteration.

It is not impoſſible by *ſlight circulating chains,* like thoſe of a jack running upon rollers, to communicate motion between ſmall ſteam-engines, placed at a conſiderable diſtance from each other; to theſe chains carriages might be connected at will, and when neceſſary they might inſtantaneouſly be detached. What a prodigious ſaving of expence might be thus effected? If the freedom and facility of intercourſe, which has been obtained by good roads and canals, be, as Adam Smith aſſerts, one of the grear cauſes of our national wealth, how far might this freedom and facility of intercourſe be extended by the perfection of the ſcheme, whoſe outlines I thus lay before the public.

Draught by ſteam engines.

Every great project requires time for conſideration; time accuſtoms the public mind to new views, and what at firſt appears too diſtant and unattainable by *time* becomes familiar and practicable.

The detail and reception of every great project requires time, &c.

Mechanics will not fail to comment on what appears in ſuch a reſpectable publication as yours. You will not, Mr. Editor, heſitate to give your own opinion; you will at leaſt be certain, that whatever objections are raiſed will be treated with candour, and replied to without enthuſiaſm, by

Your humble Servant,

RICH. LOVELL EDGEWORTH.

Edgeworth Town, Ireland.

XIV. *Deſcription*

XIV.

Defcription of the darkening Apparatus for Telefcopes by Means of Fluids. By WILLIAM HERSCHEL, *L. L. D. F. R. S.* * *with an Engraving. Plate XI.*

Conftruction of an apparatus for viewing the fun through a coloured fluid.

A B, Fig. 1. is a fquare trough, clofed at the two oppofite ends C D, by well polifhed plain glaffes. It will hold any liquid through which the fun's rays are to be tranfmitted. E is a fmall fpout, and F a handle; fo that any portion of the liquid may be conveniently poured out, when the reft is to be diluted.

The trough is made to fit into the open part of the fkeleton eye tube, Fig. 2. refting on the bottom G, and being held in its proper fituation by the fides H and I, the end K at the time of obfervation is put into a fhort tube fixed to the Newtonian telefcope, and may be turned about fo as always to have the open part H I horizontal.

When the eye-piece Fig. 3. is fcrewed by its end M into the fkeleton tube at L, Fig. 2. and the trough, Fig. 1. with any liquid to be tried is placed in the open part G H I, the fun's rays will come from the fmall mirror of the telefcope to K, and paffing through the plain glaffes C D, inclofing the liquid, will enter the eye-piece M, and after the neceffary refractions come to the eye at N. Any other fingle or double eye-piece of different magnifying powers may be fcrewed into L, inftead of the piece Fig. 3; and the liquid may eafily be tempered fo as to intercept a proper quantity of light to fuit every eye-glafs which is in ufe, and thus to render the infpection of the fun perfectly convenient.

* Philof. Tranf. 1801, p. 362, mentioned at page 21 of our Journal, but in that Number there was no room for the Plate.

XV.

On the Effects of the Respiration of the Nitrous Oxide; particularly an Instance in which the Excitation of the System produced unpleasant Symptoms. In a Letter from Mr. JAMES STODART.

To Mr. NICHOLSON,

SIR,

THE first opportunity I had of breathing nitrous oxide was in the month of June last, in company with Mr. Davy, in the laboratory of the Royal Institution. My curiosity had previously been much excited by what I had read and heard on the subject. I began breathing rather sceptically, but my doubts were soon removed. A slight degree of giddiness was followed by a pleasing thrilling and sense of warmth over the body; but not being used to breathe through a tube, and at the same time a little alarmed, I suffered a small quantity of common air to pass into the lungs, and by thus diluting the gas its powerful action was suspended. This first attempt increased my desire to breathe it in a pure state; and a few days afterwards I had another opportunity, in the company of the same gentleman. After exhausting the lungs by a forced expiration, and compressing the nostrils, I began breathing as before. The air tasted distinctly sweet; the slight giddiness was, as before, quickly succeeded by sensations very difficult to be described. I nearly lost sight of all sensible objects, and felt as if in a most delightful dream. On recovering from this trance-like state, my first feeling was certainly a sentiment of pride or disdain.—I could with difficulty descend to notice the objects around me. This emotion was succeeded by an irresistible propensity to muscular action, in which I indulged myself, by walking or rather stamping about the laboratory. It was in the morning that this experiment was made, and during the remainder of the day I felt highly exhilirated. In the evening I again breathed a quantity of the gas with nearly the same consequences. I found it impossible to resist the strong propensity to active exertions. At night I went to bed as usual, and after sound sleep I awoke in the morning more than usually chearful.

Effects of breathing the nitrous oxide.

Taste of the gas. Giddiness.

Delirium.

Recovery.

Irresistible propensity to muscular action.

No depression followed.

Other trials.

I have lately prepared and breathed the gas at different times, and the effects were always pleasurable. One circumstance I wish to notice. A pain in the right side of my face and head, which for some weeks had been very troublesome, was certainly at first increased, after breathing four quarts of the gas. The pain, however, gradually lessened, and in about an hour was quite gone, nor had I any return of it either during the remainder of the day or following night. From this I began to suspect that the gas had been instrumental in some manner in removing the pain. Some symptoms of its return during the following day induced me to give this new agent a further trial. With this view I prepared six quarts of the gas, and an hour before my bed-time I breathed it. I was soon wholly under its influence—totally lost to all sensible objects—all was visionary. I fancied a number of people in the room; imagined musical sounds; and the apartment appeared illuminated with varied coloured lights. A friend who was with me thinks I remained in this state a full minute after dropping the breathing-bag. My first enquiries were wild and incoherent; but I gradually recovered my usual temper of feeling, slept well during the night, and awoke free from pain in the face. I have felt very little of it ever since. Whether the nitrous oxide was the removing cause or not, I shall not take upon me to determine. I used no other means; nor do I know whether any other person has breathed the gas under similar circumstances. A better acquaintance with this most extraordinary agent would probably lead to important and useful discoveries.

Pain in the face mitigated and removed.

Striking phenomena of deranged perceptions. Numbers of people—musical sounds—illumination of coloured light.

Gas breathed at the Royal Institution.

Since writing the above, I have again breathed the nitrous oxide in a very pure state at the Royal Institution, and was as usual lost in pleasure. On recovering, I signified a wish to know the state of my pulse, and was told it was above 140. This was on Saturday the 13th, about three o'clock. I continued under the strong influence of the gas as to muscular action, &c. during the remainder of the day; but awoke early next morning with feelings very different to those I had formerly experienced. I felt a tremor, soon became faint, and this faintness increased so much, that for a short space of time I was as if sinking into nothing. I was certainly under considerable alarm. A mouthful of water recovered me from the apprehensions of fainting. For some hours afterwards I remained

Usual effects. Alarming consequences next day.

remained in a very low ſtate, during which I am certain that I repeatedly taſted the gas. But my mind was now perfectly tranquil, and at times I had feelings analogous to thoſe I have ſometimes experienced during the time of breathing the gas itſelf. During the whole of this and following day I felt depreſſed, with occaſional ſlight giddineſs; and as theſe feelings ſtill continued, on the Tueſday morning I conſulted Dr. Garnet, who conſidering the excitibility of the ſyſtem to have been exhauſted by too powerful a ſtimulus, directed a cordial medicine and a moderate quantity of wine, the good effects of which I ſoon felt. It required a few days, with nouriſhing diet, to recover my former ſtrength and ſpirits. I never intend again to breathe the gas in ſo large a quantity. I remain much at a loſs to account for the effects produced by the laſt doſe, ſo contrary to all my former experience; more eſpecially as Mr. Davy does not think the doſe was at all extraordinary in quantity, and the gas was not different from that uſually prepared. It has ſtood over water for a day, and was breathed at the ſame time by Mr. Davy and another gentleman with no unuſual conſequence.

Extreme depreſſion and occaſional giddineſs.

Medical treatment.

I am, dear SIR,

With much reſpect,

Your obedient Servant,

JAMES STODART.

Strand, Feb. 19, 1802.

P. S. I prepare the gas from the nitrate of ammonia; am particularly careful not much to exceed 400 degrees of heat, and have always ſuffered it to ſtand ſome hours over water before it is uſed. I procure the ſalt in a very pure ſtate from Mr. Accum.

XVI.

Remarks on the Processes for clarifying Liquids. By CITIZEN PARMENTIER *.

THE name of clarification is commonly given to a procefs by means of which liquids are deprived of fuch foreign fubftances as affect their tranfparency.

Clarifying is a procefs of fome importance.

This operation, however fimple it may be in appearance, deferves neverthelefs to be particularly attended to, efpecially when we confider the advantages it affords in the chemical and pharmaceutic arts. From thefe motives, I have thought it might be ufeful to communicate fome general notions refpecting the procefs.

Various methods.

It is not my intention to develope in this place the different methods made ufe of to obtain this purpofe, nor to fhew at length the effects they produce with regard to each of the objects fubmitted to the operation. It will be fufficient fimply to point out the principal phenomena attending operations of this kind.

Object of the procefs.

The purpofe to be anfwered, is, to clear the fluid from fuch bodies as, without being diffolved, remain fufpended, and impair its tranfparence and limpidity: but thefe bodies fometimes are feparated by repofe or filtration; in other inftances by the action of the air, of heat, of light, of motion, and of fermentation; and again in other inftances by means of agents, which, by uniting the fcattered particles in the liquid intended to be clarified, frequently change its nature, and no longer allow them to remain in their former ftate. We will begin our examination by attending to fpontaneous clarification.

Spontaneous clarifying of fluids

This takes place only when the particles to be feparated are decidedly of a lefs or greater fpecific gravity than that of the fluid in which they are fufpended. In this cafe they may unite at the bottom of the fluid, or at its furface, and form a magma, which is very eafily removed if the feparation has been complete. After this the fluid poffeffes all the tranfparence which can be defired, and the moft accurate filtration will add nothing to its clearnefs.

* Annales de Chimie.

This method of clarifying is sometimes subject to inconveniences, of which the principal are, that it requires much time, and tends by this delay to favour the formation of new products, which, by changing the composition of the fluid itself, no longer presents it, independent of the abstraction of the bodies which affected its clearness, the same as it was before its clarification. We find a very striking example of this kind when we consider what happens in the spontaneous clarification of the juices of plants or fruits. These juices when newly expressed are always turbid; they nevertheless become gradually clear; but their nature is not then entirely the same: they contain products which would not have been found if they had been clarified immediately after the expression. For this reason also it is that the juice of lemons, of gooseberries, &c. when examined before or after their spontaneous clarification, are so different in their taste, their colour, and their domestic utility.

affords time for other changes to take place;

In general we may consider it as a certain fact, that all the fermentable fluids are liable by spontaneous clarification to the effects here stated, whereas they do not take place with respect to such liquids as are little or not at all capable of fermentation, and of which the transparence is affected only by the interposition of particles incapable of acting in any manner upon the constituent parts of the fluids themselves.

and is not therefore suited to fermentable fluids.

Thus for example: Water, alcohol, ether, oil, &c. which in their first state may not have been perfectly transparent, may easily become so by spontaneous clarification, without the least change ensuing in their composition; for when we examine them after clarification, we find them in the same state as other similar fluids which have not been subjected to that process.

But other fluids are well adapted to this treatment.

The second process for clarifying fluids consists in filtering them; but this operation can never be performed without the assistance of intermediate bodies, of which the very contracted pores admit only of the passage of the fluid, and retain all the particles which were before suspended in it.

Filtration.

The instruments of filtration are exceedingly various. Paper, woollen cloths, linens, cottons, carded cotton, sponge, sand, earth, pounded glass, charcoal, porous stone, &c. all these bodies may be usefully employed in the present operation;

Different kinds of filters

tion; but their nature and their purity muſt firſt be examined more particularly when the fluid to be filtered is of a ſaline nature.

requireſelection. It is incumbent on the chemiſt to ſelect from among the different filters, that which, by producing the clarification of the fluid in the beſt manner, ſhall not at the ſame time effect any change in its conſtituent parts. Now the choice to be made in this reſpect muſt be determined from the knowledge we poſſeſs of the nature of the fluid, and that of the kind of filter proper to be employed.

Paper If the fluid be aqueous, vinous, alcoholic, or oily, paper may be uſed without inconvenience, provided it be of good quality. This laſt condition is abſolutely indiſpenſible, for without this the filtration would frequently prove defective.

acts mechanically, We know that paper is a *mechanical* texture of vegetable fibres, which have undergone different preparations. The particles of this fibre, by intertwining, leave pores, the tenuity of which is always governed by the ſtate in which the paſte or ſtuff exiſted at the moment when it was converted into paper. If this tenuity was conſiderable, the pores become ſpeedily obſtructed by the ſediment depoſited from the liquor under filtration, and at this period the filtration ceaſes. On the contrary, if the pores be very open, the filtration is made with rapidity in an incomplete manner, becauſe at the ſame time that the fluid paſſes through the pores it carries along with it the moſt minute particles which are ſuſpended, and there are only the coarſer which remain on the ſurface of the filter.

The great art is therefore to chooſe ſuch paper as has its pores of a requiſite magnitude to admit the fluid intended to be filtered, without ſuffering any of the particles which produced the turbidneſs to paſs through.

Different kinds of paper for filtering; Two ſorts of paper are found in commerce, which nearly produce this effect; and though they are not always as perfect as might be deſired, theſe have hitherto been preferred to every other. The one, which is of an imperfect white, is known in France particularly by the name of *papier Joſeph*. The other is a kind of grey paper, leſs coarſe than that which ſerves for wrapping up cheap goods. Neither of theſe have any ſize.

The

The white colour of the *papier Joſeph* ſhews that it has been manufactured with a ſuperior material to that uſed for the grey paper. The fluids which have paſſed through it are always very tranſparent, but it has the inconvenience of being eaſily torn, and its pores are ſoon obſtructed, ſo that its filtration is not effected with diſpatch.

The grey paper may be uſed for a long time alſo in producing clear fluids. But as the paſte out of which it was fabricated was not as pure as that of the *papier Joſeph*, it always communicates a diſagreeable taſte to the fluids, owing to the ſolution of the foreign matters it contains. On this account preciſely it is that certain fluids, ſuch as whey, wine, ratifia, and other liquids for beverage, when filtered through the grey paper, have always an odour and taſte, which delicate organs ſoon recognize. And hence it is that among thoſe fluids ſome of them are more ſuſceptible of alteration than when they have been filtered through the *papier Joſeph*. and their uſes.

The nature of the paper requires more particularly to be conſidered when ſaline ſolutions are to be filtered. If the grey paper be uſed, it frequently happens that part of its ſubſtance is diſſolved by the ſaline liquor, ſo that it becomes leſs pure than before. This inconvenience, which is not ſo perceptible when the *papier Joſeph* is uſed, may be ſtill further diminiſhed, by uſing the precaution of employing ſuch filters only as have been previouſly waſhed by repeated filtration of boiling water. The exact pharmacian ought to be always provided with filters thus waſhed, in order to have recourſe to them upon occaſion. Joſſe, ſo diſtinguiſhed in pharmacy, and to whom we are indebted for many important obſervations, has found the advantage of theſe inſtruments under a multitude of circumſtances. He has, among other obſervations, remarked, that whey clarified and filtered through *(du papier raiſin)* may be kept for more than fifteen days, by filtering it daily, which could not be done if it had been filtered through the common grey paper, even though previouſly waſhed.

By an effect directly contrary to this, different juices and plants are preſerved tranſparent, and in good condition, without paſſing to the acid ſtate, in conſequence of their having been daily filtered through the grey paper. It was obſerved merely that their colour became more intenſe on the firſt days, and that they afterwards became inſenſibly diſcoloured.

But

Various precautions.

But while we consider the nature of filters, their form and situation are no less important. In order that a filter of paper may produce its full effect, it must not adhere by its whole surface against that surface which supports it, for if this were the case the filtration would be soon stopped. This inconvenience is avoided by dividing it in different directions. But as the folds are soon flattened, some prefer placing between the support and the filter, straws or simple tubes of glass. I must confess that this last method has not always succeeded with me, and that I have most commonly observed, that the folds made in filters produce nearly as much effect as the pieces of straw and tubes. In Germany (and in England) they have funnels grooved or fluted within for this use.

Whatever may be the precautions which have been taken, a period will arrive at which the filtration becomes slow, and at length totally stops. This effect takes place when the pores of the paper are so much obstructed, that they no longer admit the passage of the fluid. Sometimes the filtration may be prolonged by giving a slight circular motion to the funnel; but this effect is of short duration, and there is no other remedy but to change the filter itself. It seems that there has not yet been found any means of remedying this inconvenience, which is common to all filters whatever.

Filtres of woollen;

We have before observed that filters are made of woollen cloth, of piece goods, and of carded cotton. The woollen were formerly much used, and were even the first filters adopted. They were formed into the figure of a cone, of which the base was kept open by an hoop that was fixed in a frame with supporters. This kind of filter is still used to filter ratafias, as it may be rendered very capacious, and is susceptible of receiving a large quantity of fluid at once; but it affords little, and it is therefore necessary to wait a long time before the fluid passes clear; for which reason it is seldom used, excepting when no other apparatus can be procured.

and their uses.

Nevertheless, when the filtration of syrups is required, woollen cloths are used; but then instead of giving them the form of a cone, the operator simply fixes his cloth in a square frame, fastening the four corners upon pins disposed for that purpose. The boiling syrup is poured in the middle, where it always forms a kind of concavity, and frequently after a few minutes the liquid passes very clear.

This filter may be ufed for many other fluids, particularly thofe of an aqueous nature, which do not contain potafh or foda in folution. For if they were only flightly alkaline, the filter would be foon deftroyed, and the filtered liquor would not poffefs the requifite qualities.

Linen or cotton cloths, and paper, are commonly ufed for alkaline liquids, and they fucceed very well, particularly when thofe fluids are not too concentrated. With regard to carded cotton, it is referved to filter fuch fluids as are of confiderable price, or very fcarce. **Linen or cotton cloths, or paper, to what liquids applied.**

This filter is made by introducing carded cotton into the tube of a glafs funnel, where it is lightly preffed together with a glafs rod fo as to form a kind of ftopper; after which the fluid intended to be filtered is poured into the funnel. The filtration takes place drop by drop and after the firft drops are feparated, thofe which follow are always clear. The effential oils may be very well filtered by this method, without any fear of that lofs which would neceffarily follow if the other filters before fpoken of were ufed. **Carded cotton.**

The acids, particularly thofe which are concentrated, can only be filtered through pounded glafs; but care muft be taken not to ufe this fubftance till after it has been wafhed feveral times, firft in a large quantity of water, and afterwards in an acid, in order to deprive it of the earthy fubftance which the acids might diffolve. **Filter of pounded glafs;**

Glafs filters may be very well conftructed in a funnel. The great art in order that they may produce the effect, is to fix in the firft place fome fragments of glafs in the tube, and afterwards to have others which are fmaller, and this procefs muft be continued, conftantly diminifhing the fize of the fragments until the powder lies to the thicknefs of three or four inches, the laft ftratum of which muft be very finely pulverifed. **how conftructed.**

This kind of filter operates fo well, that in lefs than an hour it is poffible to filter through a funnel of middling fize feveral kilograms of acid.

Sand is alfo very commonly employed to clarify water for domeftic ufes. Sandy fprings are in fact true filters, of which the effect is more certain, in proportion as the layers of fand are fo difpofed, that the water upon them may be obliged to pafs through the fand fucceffively, and leave thofe bodies which injure its tranfparence. **Filtring by fand.**

The

The art of conftructing fountains or fprings with fand is not yet carried to the degree of perfection it is capable of, and though it may feem to be an object of little importance, it well deferves to fix the attention of philofophers.

Obfervations.

Experience alfo fhews, that filters or fountains of fand cannot be ufed with fuccefs but for a limited time. It is neceffary to change the fand frequently, or at leaft to wafh it, in order to deprive it of the earthy heterogeneous fubftance, depofited by the water, and which when accumulated to a certain point would not only oppofe or diminifh the filtration, but alfo communicate to the fluid a flavor, which is the more difagreeable in proportion to the length of time it remains in contact with it. Nothing is more eafy, as is well known, than to deprive river water of the earth which it fufpends, and which obfcures its tranfparence. For this purpofe it requires only to be left a few hours in an earthen veffel uncovered, becaufe the action of the air is neceffary to effect this precipitation fpeedily and completely.

(To be continued.)

SCIENTIFIC NEWS.

Extract of a Letter from Brunn, in Moravia, dated January 3, 1802.

Tellurium is afferted to be antimony only.

A CHEMIST of Vienna pretends to have obferved, that the new metal *tellurium*, difcovered by Klaproth, and generally acknowledged as fuch, is nothing elfe but *regulus of antimony*: an obfervation which, according to my own experiments, has much probability.

Water faid to be decompofed by magnetifm.

Galvanifm is at prefent a fubject of occupation of all the German philofophers and chemifts. Tromfdorff has burned various metals by means of a pile of 150 plates; and at Vienna a difcovery has been made, that an *artificial magnet*, employed inftead of a Volta's pile, decompofes water equally well as that pile and the electrical machine; whence (as they write) the *electric* fluid, the *galvanic* fluid, and the *magnetic* fluid are the fame. *

* I applied the poles of a five bar magnet to two fteel wires in the tube of water, having their extremities in the water lefs than one-tenth of an inch afunder, *and perceived no effect*.---W. N.

On

On the Muriatic Acid.

Chenevix on the ſtates of the muriatic acid.

The reading of Mr. Chenevix's paper upon oxigeniſed and hyperoxigeniſed muriatic acid, before the Royal Society, was concluded on Thurſday the eleventh.

After a ſhort account of the experiments that had been made before him, and particularly of the ingenious conjecture of Mr. Berthollet,* he ſtates the means by which he has aſcertained, that the acid contained in hyperoxigeniſed muriate of potaſh is muriatic acid, in a particular ſtate of combination with oxigen; and the experiments by which he determined the proportion of thoſe elements. From the quantity of oxigen and of ſimple muriatic acid contained in the ſalt, he proves that hyperoxigeniſed muriatic acid conſiſts of

Oxigen	65
Muratic acid	35
	100

From the proportions of the ſalt which is formed when a current of oxigeniſed muriatic acid is paſſed through a ſolution of potaſh, and which he found to be compoſed of the ſame elements, and in the ſame proportion as oxigeniſed muriate of potaſh would be, if at the very moment of its formation it had not been reſolved into ſimple and hyperoxigeniſed muriate, he concludes that oxigeniſed muriatic acid is compoſed of

Oxigen	16
Muriatic acid	84
	100

From a number of experiments, which are ſtated at length, Mr. Chenevix imagines that the ſalts of the genus oxigeniſed muriate, do virtually exiſt; but that by ſuperior affinities they are reſolved into muriate and hyperoxigeniſed muriate at the very moment that the acid comes into contact with the baſes.

He deſcribes and analyſes the ſalts of the genus hyperoxigeniſed muriate; and mentions the hyperoxigeniſed muriate of ammonia as an extraordinary inſtance of diſpoſing affinities.

* Journal de Phyſique, 1788, page 217.

He

Chenevix on the ſtates of the muriatic acid.

He obtained the alkaline ſalts pure by repeated cryſtalliſations, and the earthy ſalts by boiling them with phoſphate of ſilver.

He has examined likewiſe ſeveral of the metallic ſalts of this genus, and mentions hyperoxigeniſed muriate of ſilver, as ſtrongly marking the difference between muriatic and hyperoxigeniſed muriatic acid. He aſcribes to this ſalt, when mixed with combuſtible bodies, an expanſive force, which he thinks he will be much within bounds, if he ſtates to exceed five times that of any known detonating ſalt.

He concludes with an appeal to the chemical world, whether, in the preſent ſtate of the ſcience, it would not be more philoſophical to ſay,

Muriatic radical, or ſome one word analogous to it,	Inſtead of	Muriatic acid.
Muriatous acid,		Oxigeniſed muriatic acid.
Muriatic acid,		Hyperoxigeniſed muriatic acid.

and ſtates the arguments in favour of either mode of appellation.

As a warning to thoſe who would repeat his experiments he relates, in the courſe of his paper, an accident that happened in his laboratory to himſelf and to Mr. Vandier, by which the latter gentleman had almoſt loſt his ſight, and was wounded in the moſt dreadful manner.

A new Method of obtaining the Gallic Acid pure. By M. Fiedler, *Student in Pharmacy* *.

As alumine has a great affinity with many vegetable ſubſtances, I inferred that this property might be applied to diſengage the gallic acid from the extractive and the tanin which always adhere to it; and by that means to obtain the acid pure and eaſily cryſtallizable.

Solution of alum precipitated by potaſh.

Experiment 1.—I diſſolved two ounces of alum in boiling water; I filtrated and precipitated it by a ſolution of potaſh; I ſeparated the precipitate, and edulcorated it till the water of the lixiviation no longer rendered the muriate of barytes turbid.

* Journal de Chimie, par Van Mons. I. 85.

Experiment

Experiment 2.—I infufed, to a reduction of one half, an ounce of good gall nuts in fixteen ounces of water. The refult was a brown infufion, which after many filtrations continued turbid.

Nut-gall infufed in water.

Experiment 3.—I mixed with the preceding infufion the precipitate of alumine, and frequently agitated the mixture with a glafs rod. The next day I filtered, and obferved that the matter paffed perfectly clear, which convinced me that the alumine had precipitated the extractive. I wafhed the precipitate with warm water, until the water no longer blackened fulphate of iron, after which I left it to dry.

The precipitate of alum, being added to the infufion, combined with the extractive part.

Experiment 4.—As the alumine had evidently feparated the extractive fubftance from the infufion of gall nuts, I could only fuppofe that the liquor of the filtration muft contain tanin at the fame time with the gallic acid. I afcertained the prefence of the latter, by a folution of fulphate of iron; and to difcover the former, I proceeded in the following manner.

The clear fluid might contain tanin and gallic acid.

Experiment 5.—I diffolved two fcruples of ifinglafs in an ounce of water, and poured a few drops of this folution into the liquor of the gallic acid; they did not make it turbid in the leaft; which proved that it did not contain any tanin, which, with the gelatine, ought to have formed an elaftic fubftance, infoluble in water. As the tanin was not difcoverable in this liquor, there remained no doubt of its having combined with the alumine, which I afcertained in the following manner.

But, by the teft of ifinglafs, it contained no tanin. Confequently the tanin was combined with the alumine of Exp. 3.

Experiment 6.—I digefted a portion of the precipitate of the infufion of gall nuts with diluted fulphuric acid. It was entirely diffolved. The liquor paffed clear through the filter, but coloured by the extractive matter. I dropped in a little of the ifinglafs folution, and immediately obferved white filaments floating in the liquor, which by agitation formed into a mafs, poffeffing all the properties of tanned glue; namely, of becoming elaftic by heat, of foftening by water, infolubility in alcohol, &c.

Precipitate of No. 3. was diffolved in fulph. acid, which feized the alumine, while the tanin was precipitated by mucilage.

Experiment 7.—The filtered liquor, as well as the water of the lixiviation in the third experiment, of courfe held in folution nothing but the gallic acid. I concentrated the two fluids by flow evaporation, and obtained a falt in fine needles, which was the pure gallic acid.

The clear folution of gallic acid, Exp. 4. afforded fine cryftals of gallic acid.

The

The Seven following Articles are Extracts of a Letter of CIT. VAUQUELIN *to* CIT. VAN MONS *.

1. *Discovery of the Chromate of Iron in France.*

Chromate of iron in France.

The chromate of iron has been found in abundance in the department of Var: we can now prepare any quantities of this metal, and study its properties in detail more than we have hitherto been able to do. Above all, we can compose in a direct way the chromate of lead, which is a very interesting object in painting. The chromate of iron may also be used to form a beautiful green on porcelain or artificial stones.

2. *Discovery of the Emerald.*

Emerald in France;

In a mineralogical tour which Cit. Lelievre, member of the Council of Mines has just made, he has discovered a quarry of emeralds, which are so abundant, that they are used in the country to pave the roads. They are found in the environs of Limoges mixed with granite, often without any regular form, and sometimes crystallized, but their colour and transparency are not beautiful.

3. *Discovery of the Neutral Phosphate of Iron.*

and neutral phosphate of iron.

Cit. Lelievre has likewise discovered in the same place a mineral sufficiently interesting, namely, a perfect phosphate of iron, that is to say, that the iron is intirely saturated by the phosphoric acid, a combination which had not hitherto been found in nature; it has a red brown colour, is semi-transparent, and has a foliated texture.

4. *On the Zoonic Acid.*

The supposed zoonic acid is the acetous;

Cit. Thenard has just finished an examination of the zoonic acid, in which he appears to have demonstrated that it is nothing but the acetous acid combined with a peculiar animal matter.

5. *On the Cobaltic Acid.*

and the cobaltic acid is arsenic.

Cit. Darracq has read a paper to the Institute, in which he shews that the acid found in zaffre by Cit. Brugnatelli, and which he named the cobaltic acid, is absolutely nothing but the arsenic acid.

You see that by degrees the number of chemical substances diminishes, which is a great advantage to the science.

* Journal de Chimie, par Van Mons. I. 218.

ACCOUNT

ACCOUNT OF BOOKS OF SCIENCE.

Elements of Chemiſtry. By J. Murray, *Lecturer on Chemiſtry, &c.* Edinburgh, 1801. Longman and Rees, London.

THE author of this work appears to be well acquainted with every thing at preſent known relative to the ſcience of chemiſtry. He follows the claſſification of ſubſtances according to the order of their ſimplicity, and has given a digeſted, conciſe, and perſpicuous detail of every important fact concerning their chemical relations. Murray's elements of chemiſtry.

The view he takes of the preſent ſtate of our knowledge reſpecting the important ſubject of heat is clear, candid, and ſatisfactory. On this, as well as on other controverted points, after ſtating the leading arguments on both ſides he preſents his own opinion where requiſite, in the modeſt form of a query. On the whole, this work may be conſidered as a ſafe and clear guide to the ſtudent of chemiſtry; and as an uſeful courſe previous to the ſtudy of the more extenſive ſyſtem of Fourcroy. B. B.

A Syllabus of a Courſe of Lectures on Natural and Experimental Philoſophy. By Thomas Young, *M.D. F.R.S. Profeſſor of Natural Philoſophy in the Royal Inſtitution of Great Britain.* Quarto, 193 pages. From the preſs of the Royal Inſtitution, where the work is ſold, and at Cadell's, in the Strand, London.

This ſyllabus appears to be one of the moſt complete and accurate outlines of ſcience which has been hitherto publiſhed. It is divided into four parts, and each of theſe into ſubordinate ſections, conſiſting of paragraphs regularly numbered. I cannot pretend in this notice to enter into any diſcuſſion of the numerous ſubjects which, in the nature of things, muſt be very conciſely ſtated in ſuch a work, and of which the illuſtrations muſt be ſought in the lectures of the learned profeſſor; but ſome intimation of the arrangement and objects of inſtruction will be gathered from the titles as follow. Part I. Mechanics. Of induction; motion; compoſition of motion; centre of inertia and of momentum; accelerating forces; central forces; projectiles; motion in given ſurfaces; colliſion Young's ſyllabus of philoſophical lectures.

Young's ſyllabus of philoſophical lectures.

and energy; preſſure; equilibrium; rotatory power; preponderance; practical mechanics in general; geometrical inſtruments; perſpective; ſtatics; friction; paſſive ſtrength; architecture; carpentry; wheel-work; union by twiſting; economy of motion; time-keepers; raiſing weights; friction wheels; carriages; compreſſion and extenſion; penetration and attrition; trituration and demolition. Part II. Hydrodynamics. Of hydroſtatic equilibrium; floating bodies; ſpecific gravity; pneumatic equilibrium; hydraulics; reſiſtance of fluids; hydraulic machines; pneumatic machines; ſound; harmonics; properties of light; dioptrics and catoptrics; optical inſtruments; phyſical optics; nature of light. Part III. Phyſics. Of the fixed ſtars; the ſun; primary planets; ſecondary planets; comets; laws of gravitation; ſenſible effects of the celeſtial motions; practical aſtronomy; geography; tides; general properties of matter; heat; electricity; magnetiſm; meteorology; natural hiſtory. Part IV. Mathematical Elements. Of quantity and number; ſpace; the compariſon of variable quantities; the properties of curves; practical rules and tables.

The whole work is beautifully printed, and ſuch demonſtrations as occur are diſtinguiſhed from the narrative or enunciation by a ſmaller type. The figures are extremely neat. A fifth part is mentioned as probably hereafter to appear, containing a catalogue of the beſt authors.

Mr. BLAIR's POPULAR LECTURES.

Mr. Blair has recently commenced a courſe of popular lectures, at the Bloomſbury Diſpenſary, for the information of ſcientific perſons, amateurs of natural hiſtory, and ſtudents in the liberal arts. He propoſes to explain and illuſtrate the following ſubjects, on Tueſday evenings, at eight o'clock:

On the component parts of the body—the bones, cartilages, and ligaments—muſcles, and muſcular action—the integuments and membranes—brain, nerves, and ſenſation—the heart and vaſcular ſyſtem—the blood, circulation, and abſorption—glands, ſecretions, and excretions—reſpiration and animal heat—digeſtion, nutrition, and growth—utero-geſtation, and parturition—the eye, and phenomena of viſion—functions of the ear, noſe, and mouth—phyſiognomy, beauty, and the paſſions.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1802.

ARTICLE I.

Experiments on the Transmission of Heat downwards through Mercury and through Oil contained in Vessels of Ice; by which those Fluids are proved to be proper Conductors of Heat. By Mr. JOHN MURRAY, *Lecturer in Chemistry, Materia Medica, and Pharmacy, at Edinburgh. Communicated by the Author.*

Review of the experiment of transmitting heat downwards through a fluid.

IN a former Memoir * I related several experiments made with the view of determining the question, whether fluids are capable of conducting caloric, and stated the sources of fallacy by which any strict conclusion from these is invalidated. The experiment calculated to determine this question with greatest certainty is that of heating a fluid downwards by bringing a hot body into contact with its upper surface; but in making this experiment a source of error is always present in the conducting power of the vessel in which the fluid is contained. When the upper portion of fluid has its temperature raised by the application of a hot body, part of it must flow towards the sides of the vessel, and give out part of its caloric; the solid

* Philos. Journal, I. 165. where by mistake the author's titles are erroneously given.---W. N.

matter of the veſſel muſt convey more or leſs of this caloric downwards, and communicate it to the fluid beneath; and thus caloric may appear to be conducted to a conſiderable depth, though the fluid do not actually poſſeſs a conducting power. In any uſual mode of performing this experiment, this ſource of fallacy cannot be intirely obviated; nor can its effects be eſtimated with ſuch accuracy, as to determine preciſely how far any obſerved riſe of temperature in a fluid is to be aſcribed to its operation.

Error from the veſſel, avoided by forming it of ice.

It occurred to me, that this error might be completely avoided by employing a hollow cylinder of ice to contain the fluid. Suppoſe a thermometer to be placed horizontally within ſuch a veſſel, its bulb being in the axis of the cylinder, and covered with a fluid at the temperature of 32°. Let a hot body be brought in contact with the upper ſurface of the fluid, or even immerſed in it, but ſtill ſo as to be at ſome diſtance from the bulb of the thermometer; if the thermometer riſe it may be concluded that the fluid has a conducting power, ſince the caloric could reach the bulb in no other mode. Caloric does not paſs through fluids by radiation; it is eaſy to perform the experiment ſo as to prevent the thermometer from being heated by any motion of the fluid, from immerſing the hot ſolid; and laſtly, no caloric could be tranſmitted by the ſides of the veſſel, ſince ice cannot have its temperature raiſed above 32°, and cannot therefore communicate any temperature above that to a contained fluid. The concluſion therefore ſeems undeniable, that if in ſuch an experiment the thermometer riſe, caloric muſt be conveyed to it by the fluid interpoſed between it and the heated ſolid.

If the thermometer do riſe the fluid is a conductor.

Whether the oppoſite concluſion may be affirmed, in caſe the thermometer do not riſe?—No.

It ſeemed doubtful, however, whether the converſe of the propoſition would hold equally good. If the thermometer were not to riſe, would this prove that the fluid is a perfect non-conductor? It is not evident *a priori*, whether a fluid contained in a veſſel of ice is capable of being heated above 32°, or at leaſt could convey caloric above that temperature to any perceptible diſtance, even allowing it to have a conducting power. Suppoſe that the heated ſolid is immerſed in the fluid, the particles in contact with it will be heated, and will form an aſcending current which muſt flow towards the ſides of the veſſel. The ice will abſtract the exceſs of caloric from theſe heated particles, and they will return to the tem-

perature

perature of 32°. It appears doubtful, therefore, whether any part of the fluid beneath the heated solid, supposing it to be capable of conducting caloric, can have its temperature in such a situation raised; and hence it appears uncertain, whether from the circumstance of the thermometer not rising in such an experiment, it could fairly be concluded, that the fluid is a perfect non-conductor of caloric.

If the heat be given out faster than the ice can absorb it, the thermometer will rise *if the fluid be a proper conductor.*

There are also reasons, however, which render it probable, that any conducting power in the fluid if it do exist, may be discovered by an experiment of this kind, by the rise of the thermometer. Supposing the fluid to possess such a power, it is sufficiently possible that the caloric may be conveyed from the heated solid through the interposed fluid, (especially if the quantity of the fluid be small) to the thermometer, more quickly than it can be cooled by the ice. Two facts render probable this supposition. 1st. Ice is an imperfect conductor, and therefore absorbs caloric slowly from another body. If a piece of ice be thrown into water at 40 or 50°, a considerable time elapses before the temperature of the fluid is reduced to 32°; and 2dly. The celerity with which a hot body gives its caloric to a cold one, is, *cæteris paribus,* proportioned to the difference between their temperatures. If the hot body, for example, have a temperature 100 degrees higher than the cold one, it will communicate caloric to it with much more celerity, than if it were at a temperature only five degrees higher. This cause operates in the present experiment. The difference between the temperature of the heated solid suspended over the thermometer, and that of the fluid interposed is very considerable, and it will therefore give caloric to it rapidly. On the other hand, the difference between the temperature of the particles of the fluid that are heated, and that of the ice with which they come in contact must be much less, and therefore they will part with their caloric more slowly. From this circumstance, independent of the slowness with which ice absorbs caloric, the communication of caloric in this experiment must be more rapid than its abstraction; and therefore if the fluid do possess a conducting power, the thermometer must be heated.

Whether the stratum of heated fluid can extend below the hot body, and thus

In reflecting on this subject it seemed even possible, that the fluid might be cooled so much more slowly by the sides of the vessel, than it was heated by the solid suspended in it, that the heated

raife the thermometer?—Not unlefs the veffel conducts; which in this cafe it does not.

heated particles might accumulate, and perhaps at length come in contact with the bulb of the thermometer, raife its temperature, and thus give the appearance of the fluid poffeffing a conducting power, though it had none. I was foon fatisfied, however, that this could not be the cafe. Suppofing the caloric to be ever fo flowly abftracted from the hot particles, ftill the ftratum of fluid directly heated, can never extend beneath the folid which is the fource of the caloric. It is from it that the current of heated particles afcend, and they defcend by the fides of the veffel, where their temperature may be reduced partially or intirely. But this defcending current cannot proceed lower than the point where the afcending one commences, and of courfe no part of the fluid which has thus been directly heated can reach the bulb of the thermometer. In glafs veffels this is not the cafe, becaufe in thefe caloric is given out by the fides of the veffel, even beneath the heated folid, which is fufpended within it. But in an ice veffel there is no communication of this kind, as it cannot conduct caloric at any temperature above 32°.

But if the thermometer rifes, the fluid conducts; and if not, the fluid is probably a non-conductor.

The experiment then which has been ftated is fubject to none of thefe fources of fallacy. If the thermometer rife, there can be no doubt that the fluid conducts caloric. If it do not rife, it may be concluded, if not with equal certainty, at leaft with the greateft probability, that it has no conducting power.

A veffel was formed of ice.

A quantity of water was frozen in a tin mould, fo as to form a hollow cylinder A, Pl. XIII. Fig. 2. the diameter of the cavity of which was three inches, and the depth $3\frac{1}{2}$ inches, the thicknefs of the ice forming its fides and bottom, being $1\frac{1}{2}$ inch. A thermometer B was introduced into it horizontally at the depth of one inch, the bulb being in the axis of the cylinder, and was frozen in, fo that the veffel was capable of containing any fluid. It is evident that by pouring a fluid into this ice cylinder, fo as to cover the bulb of the thermometer to a greater or lefs height, and by fufpending in it a heated folid, any propagation of caloric through that part of the fluid beneath the folid, will be afcertained by the rife of the thermometer.

Water could not be ufed in thefe experiments, becaufe it contracts by heat

For making this experiment with a view to determine the conducting power of fluids, water is altogether unfit, from the fingular property it has of expanding inftead of contracting in every reduction of temperature from 40° of Fahrenheit to 32.

Suppofe

Suppose the cylinder of ice to be filled with water at 32°, and a heated solid to be suspended in it, the portion of water in contact with the solid, will immediately have its temperature raised perhaps two or three degrees. But by this increase of temperature it is not expanded, but is contracted in volume, it becomes heavier, and must therefore sink in the fluid towards the bulb of the thermometer; it will be succeeded by another heated portion, a descending current must thus be formed, and the thermometer will have its temperature raised by the contact of particles which had been directly heated by the suspended solid.

near 32°, and would descend to the thermometer.

Oil and quicksilver are not liable to this source of fallacy. It has been ascertained by Crawford and De Luc, that the diminutions of volume in these fluids from decrements of temperature are nearly uniform, or at least at any temperature necessary for performing experiments of this kind they contract with sufficient regularity, as is indeed sufficiently shewn by their use as thermometrical fluids—With these fluids therefore the following experiments were made:

Oil and mercury do not contract by heat; at least about the temperature here mentioned.

Experiment I. A quantity of almond oil was poured into the ice vessel, so as to cover the bulb of the thermometer one quarter of an inch. A small iron cup C, cylindrical, two inches in diameter, and flat in the bottom, capable of holding two ounces by measure, was suspended so as merely to touch the surface of the oil, and was filled with boiling water. This was preferred to a solid ball, as presenting a larger surface to the oil, and as it was more easy to ascertain the precise depth at which it was immersed. At the beginning of the experiment the thermometer stood at 32°. In a minute and a half it had risen to $32\frac{1}{4}$, in three minutes to $34\frac{1}{2}$, in five minutes to $36\frac{1}{4}$, in seven minutes to $37\frac{1}{2}$. At this point it became stationary, having risen $5\frac{1}{2}$ degrees of Fahrenheit's scale in seven minutes. The temperature of the water in the cup had in this time fallen to 96°. The thermometer, after remaining stationary at $39\frac{1}{2}$ for six minutes began to fall, and it continued to descend at the rate nearly of a degree in a minute and a half, till it returned to 32. At the conclusion of the experiment the sides of the cylinder at the upper part were partly excavated, the excavation was greatest at the surface of the oil, it became less as it descended, but it had taken place in a slight degree, even lower than the bulb of the thermometer,

Experiment I. Almond oil at 32° in the ice vessel was heated by water poured into a stationary metallic vessel slightly immersed in it. *The heat descended* to a thermometer.

Excavation of the ice.

meter, ſhewing that the fluid had been heated to this depth. The oil ſtill covered the thermometer one quarter of an inch.

Exp. II. Repetition with a greater depth of oil.

Experiment II. The experiment was repeated, the oil covering the thermometer half an inch. The temperature was as formerly 32°, in three minutes the thermometer had riſen to 32¼, in ſix minutes to 32¾, in eight minutes to 33, in 12, to 34, in 15 minutes to 34½, at which it became ſtationary. In this experiment, therefore, the thermometer had riſen 2½ degrees in 15 minutes: it deſcended as ſlowly as in the preceding experiment. In a third experiment in which the thermometer was covered three quarters of an inch with oil, it roſe only 1½ degree in the ſame time.

Exp. III. Repetition with mercury. Speedier tranſmiſſion of heat.

The ſame experiment was next repeated with quickſilver in a ſimilar cylinder of ice. 1ſt. The quickſilver covered the bulb of the thermometer one quarter of an inch. The ſmall iron cup was not ſuſpended, but allowed to float on its ſurface, and was filled with boiling water gently poured in. The thermometer began to riſe inſtantly: in one minute it had riſen from 32° to 36°, it remained at that temperature, or at leaſt with the increaſe of half a degree, for another minute, and then began to fall; in three minutes it had fallen to 35, the temperature of the water in the cup having fallen to 102. The thermometer continued to deſcend ſlowly, till it returned to 32°.

Whether the ſubſidence of the mercury from fuſion of the ice can affect the concluſion?

In this experiment there is a particular ſource of fallacy, againſt which it was found neceſſary to guard. From the melting of the ſides of the cylinder of ice at the upper part by the contact of the heated fluid, the diameter of the cavity is enlarged, and therefore the column of mercury muſt diminiſh in height. In the experiment with the oil this does not take place, becauſe the water formed from the melting of the ice being heavier, falls by the ſides to the bottom, and ſupports the column of oil at its preciſe height, the one circumſtance counterbalancing the other, ſince as much ice as is melted, as much water is produced, and very nearly the ſame volume is occupied by both. It was accordingly aſcertained by exact meaſurement at the end of each experiment with the oil, that it had not ſunk perceptibly; in other words, the bulb of the thermometer remained covered with the ſame height of fluid, as at the commencement of the experiment. But with the quickſilver the caſe is otherwiſe; the water produced by the

melting

melting of the ice floats on its surface, and hence from the enlargement of the diameter of the vessel, the height of the column of mercury must be diminished. It was therefore necessary to ascertain with accuracy, whether this diminution had proceeded so far, as that the particles of mercury which had been in contact with the bottom of the iron cup, and directly heated by it, had come in contact with the bulb of the thermometer. This was found not to be the case. At the end of the experiment the bulb was still covered with so much mercury that the cup floated freely; and it was ascertained in particular both by actual measurement, and by a gage frozen in the side of the cylinder, that the quantity of mercury covering it was still one-eighth of an inch deep. As therefore the cup merely floated on the surface, the part of the fluid *directly* heated by it could not have come in contact with the bulb. This was likewise clearly established by the phenomena of the experiment itself; the thermometer rose rapidly at the commencement of it before the quicksilver could have sunk sensibly, and it remained stationary at the end when the full descent had taken place, although the water in the cup was still warm. On pushing it down so as to cause it to touch the bulb the thermometer rose rapidly, a proof that it had not previously been in contact with it. In this point of view therefore the experiment was unexceptionable, and those which follow were still less liable to any fallacy of this kind, as a larger quantity of fluid was interposed between the heated body and the thermometer.

Proofs that it did not.

2d. As much mercury was poured into the vessel of ice as to cover the bulb of the thermometer half an inch, and water at 212° was poured into the iron cup floating on its surface. The thermometer stood at 32, in one minute it rose to $32\frac{2}{4}$, in two minutes to $33\frac{1}{4}$, in three minutes to $33\frac{1}{4}$, it then became stationary, and in six minutes more began to fall. The quicksilver in the vessel was found to have sunk rather less than one quarter of an inch. The thermometer therefore at the end of the experiment still remained covered with one quarter of an inch of fluid.

Exp. IV. Repetition with a greater depth of mercury.

3d. When the experiment was made a third time, the bulb of the thermometer being covered with one inch of quicksilver, the rise of temperature amounted to three-fourths of a degree.

Exp. V. and still greater.

All

Variation of the circumstances.

All these experiments were frequently performed, they nearly agreed, and the above are the average results. Every precaution was taken to ensure accuracy, in which I was assisted by several friends. They were likewise occasionally varied. In one instance the cylinder of ice was made so wide, that a small thermometer was placed entirely within it in a horizontal position, and covered with oil, but the result was nearly the same. In others a solid brass ball was suspended in the fluid instead of the cup with hot water, but without any material difference in the result. In constructing the vessel it was found necessary that the ice should be frozen hard, that it might contain the fluid without allowing any of it to transfude; and in the experiment with the mercury the external mould of tin was allowed to remain round the ice cylinder to give it strength, and enable it to contain the mercury without any risk. This however could make no difference in the experiment, and the result was found to be the same when the tin was removed.

Developement of the process.

In all these experiments then, a rise of temperature in the thermometer took place, greater or less, proportioned to the quantity of fluid interposed between it and the heated body. From the nature of the experiment this rise could not be expected to be considerable, since the fluid when heated must quickly have had its caloric abstracted by the cylinder of ice in which it was contained. Hence at a certain stage of the experiment the thermometer became stationary, though the heated body suspended over it still retained a considerably high temperature, the communication of caloric to the fluid not being equal to its abstraction by the ice; and when the communication became less rapid than the abstraction, the thermometer began to descend. In all these experiments, however, even in those in which the largest quantity of fluid was interposed, the rise of the thermometer was unequivocal, and in some of them, considering the circumstances of the experiment, very considerable.

Conclusion. *The fluids were direct conductors of heat.*

This rise it appears to me impossible to ascribe to any other cause, than to a power in the fluid to conduct caloric. Any other that might be imagined can be proved not to exist.

For the sides of the vessel could not conduct:

Thus it is evident, that the sides of the vessel could not convey to the fluid in contact with the bulb of the thermometer, any part of the caloric it received. Ice in common with

with any other ſolid may at temperatures below its melting point conduct caloric ; but as it cannot poſſibly exiſt with a temperature above 32°, it cannot communicate any temperature above that to a fluid in contact with it, and conſequently it could not contribute in the above experiments to raiſe the thermometer above that temperature.

The experiment was performed in ſuch a manner that no motion was occaſioned in the fluid capable of conveying any part of it directly heated to the bulb of the thermometer ; and indeed the regular, and in ſome of the experiments, the ſlow riſe of the thermometer is inconſiſtent with the ſuppoſition of caloric being communicated by any cauſe of this kind. neither was the fluid agitated ;

It has been proved, that any current produced in the fluid by the contact of the heated ſolid could not be the mean of conveying caloric ; for if the fluid be ſuppoſed to have no conducting power, ſince the ſides of the veſſel are likewiſe incapable of communicating any increaſe of temperature, the portion of fluid beneath the ſurface could not poſſibly be diſturbed by a heated ſolid merely reſting on that ſurface. Even if the ſolid were immerſed to ſome depth in the fluid, it is only from the ſides and under part of it that a current could riſe, and of courſe no part beneath that level could be affected. nor could any heated current deſcend ;

It has likewiſe been ſhewn, that the riſe of the thermometer cannot be aſcribed to the contact of particles directly heated by the ſolid, and reaching the bulb from the ſinking of the column of fluid by the enlargement of the diameter of the veſſel from the melting of the ice ; 1ſt. becauſe in the experiment with the oil this ſinking did not take place, the water produced ſupporting the oil at the ſame height ; and 2dly. becauſe even in the experiments with the mercury, the dimimution did not take place to that extent which would have been neceſſary to have brought the particles which were heated on the ſurface of the fluid into contact with the thermometer, as was proved by actual meaſurement, and by the phenomena of the experiment itſelf. nor did the maſs of warmed fluid ſubſide to the thermometer ;

Laſtly, the caloric which reached the bulb of the thermometer could not have been propagated by radiation, becauſe caloric, it has been proved, does not radiate through tranſparent fluids ; and it cannot even be ſuppoſed capable of paſſing by radiation through an opaque fluid as mercury. The laſt point is obvious, and is of itſelf deciſive with reſpect to this nor was the heat tranſmitted by radiation.

objection.

objection. But even the first, that caloric does not radiate through a transparent fluid, has been sufficiently proved by the experiments of C. Rumford. I shall add one of a similar kind made with the particular apparatus used in the preceding experiments, which appears to me sufficiently decisive. The thermometer in the ice cylinder was covered with oil one quarter of an inch, and the cup was suspended over it in such a manner that the bottom of it, though near to the surface of the oil, *did not touch* it. Boiling water was poured in, the thermometer rose scarcely one degree in five minutes, and did not rise more in any longer time. It has been shewn that when the thermometer was covered with one quarter of an inch of oil, and the cup filled with warm water, was suspended so as to *touch* the surface, it rose in seven minutes 5½ degrees. Had this rise been owing to the communication of caloric by radiation, the variation in the experiment which has just been related, could not have produced any change in the result. The communication of temperature by the *conducting power* of the fluid, must be facilitated by the contact of the fluid with the heated solid, but the communication of it by *radiation* required no such aid, nor could it be promoted by it. The mere circumstance of the heated solid not touching the fluid, could make no difference with respect to the one, but must make the most essential difference with regard to the other, and the experiment clearly proves, that even through transparent fluids caloric does not radiate. The slight rise of temperature that took place, is owing to the caloric passing partly by *radiation*, and partly by communication by the medium of the air to the *surface* of the fluid, and from that it is conveyed by the conducting power of the fluid to the thermometer. The proof of this is, that even with mercury, a slight rise of temperature was indicated when the experiment was performed in the same maanner. When the cup with hot water was suspended over the mercury covering the bulb one quarter of an inch, but not touching it, the thermometer rose one degree.

Experiments of radiation; with remarks.

Concluding remarks.

It may therefore from the whole of these experiments be concluded, that the observed rise of temperature was owing to the fluid conducting the caloric, since no other cause can fairly be assigned. I have not subjected any other fluid to experiment, because it was difficult to find any of a nature sufficiently different, which the ice would have been capable

of

of containing. But if it be proved that oil and mercury are capable of conducting caloric, it will be admitted as sufficiently probable that other fluids muft have a fimilar power. Of thefe two, mercury it is probable, from thefe experiments, is the beft conductor, as the rife of the thermometer took place in it much more rapidly than in the oil. That the temperature did not rife higher in it than in the oil, feems to have been owing partly to its greater mobility, by which its parts would move towards and from the ice with more celerity, and partly to its better conducting power, by which it would give out its caloric more quickly to the large fuperficies of ice with which it was furrounded. It was accordingly obferved, that the fides of the ice cylinder were more excavated in the experiments with the mercury than in thofe with the oil.

Edinburgh, Feb. 27, 1802.

II.

Defcription of a new Efcapement for Watches, invented by Mr. JOHN DE LAFONS *.

THE inventor ftates, that fince the perfection of chronometers confifts more in the equality of impulfe given to the balance than to any other caufe, he has contrived the prefent efcapement for giving fuch an impulfe; with the additional qualities of locking the wheels without fpring work perfectly fafe, and unlocking them with lefs confumption of power than in any other efcapement he knows, becaufe the wheels do not bear againft the locking with more than one tenth part of the whole preffure from the main fpring, which laft circumftance he conceives to be perfectly new.

The perfection of chronometers depends chiefly on the equality of the impulfes given to the balance.

He remarks, that the equality of impulfe has been effected with great ingenuity by Mr. Mudge and Mr. Haley; but at the fame time he objects to the efcapement of the former, on account of its extreme difficulty and expence, and to that of the latter, for its very compound locking. And he alfo re-

Efcapements of Mudge and Haley.

* From the Tranfactions of the Society of Arts. 1801. A premium of thirty guineas was given by the Society to the inventor: I have not copied the letter prefs, as I wifhed to render the defcription rather more minute than was neceffary in that place.—N.

plies

plies to ſome obſervations which were made at the committee of the Society, which do not require to be ſtated here.

Deſcription of a new eſcapement. Its parts.

Plate XIV. exhibits the eſcapement, where the ſame letters denote the ſame things in all the figures. A is the ſcape-wheel, B a pallet lever on an arbor with fine pivots, having at its lower end, Fig. 1 and 2, a remontoire, or ſpiral ſpring, C, fixed with a collar and ſtud as pendulum ſprings uſually are. H H repreſents the balance, having upon its verge K K, a pallet D, conſiſting of two arms, which carry a ſmall roller moveable on fine pivots; and alſo certain other pallets E, repreſented more fully in Fig. 4, the plan of which parts is ſeen in Fig. 1, at E and D. F is an arm which ſerves to move two locking pallets *a* and *b*. It is continued beyond the center of motion, in order that it may balance itſelf in all poſitions, and at the part moſt remote from the balance there are a couple of ſtuds and ſcrews to adjuſt and bank the quantity of motion. The laſt mentioned pallets have their faces, portions of circles, and they move on an arbor with fine pivots, as may be ſeen at Fig. 2 and 4, in which figure alſo it is to be obſerved, that the arm of the locking pallets is formed into a triple fork, the branches of which are expoſed to the action of two pallets E before mentioned.

Performance of the eſcapement. 1. The ſcape wheel winds up a lever and ſpring; and is caught by a detent. But the re-action of the ſpring lever renders the hold of the detent exceedingly light. 2. The balance unlocks the wheel which lets go the lever, and this ſtrikes a pallet on the balance arbor. 3. In its return the balance again unlocks the wheel, which again winds up the lever, (No. 1, 2, 3, &c.)

From the above deſcription, and the conſideration of the figures it will be apprehended, that, ſuppoſing the maintaining power to urge the wheel A forward, and the balance to be ſet in motion by external means, the lever pallet B will be wound up to its poſition in Fig. 1. by the tooth 1 of the wheel, at which inſtant the tooth 3 reſts upon the pallet *a*. Suppoſe the balance to move in the direction of the ſmall arrow near D, the verge pallet D will juſt paſs the end of the lever pallet B, at which moment one of the two unlocking pallets E will carry before it the arm F, and unlock the pallet *a*, and conſequently the wheel will drop with its tooth 4 upon *b*, and the lever pallet B will be ſet at liberty by the advance of the tooth 1, to act upon the balance by the pallet D, until B is ſtopped againſt the tooth 2, as ſeen in Fig. 5. By this means the balance receives the ſame invariable impulſe at every ſecond vibration. In its return the oppoſite unlocking pallet takes the other external arm of the fork of the locking pallets, and by that means unlocks *b*. The wheel immediately drops forward, almoſt through the ſpace of one tooth,

tooth, fo that the tooth 1, Fig. 5, falls upon the pallet *a*, while the tooth 5 winds the lever pallet B again into the pofition exhibited in Fig. 1. And the balance upon its return receives a fecond impulfe under the circumftances already detailed.

It remains only to be fhewn in what manner the middle prong of the fork is employed. Its ufe is to fecure the locking of the pallets *a* and *b*. When the unlocking is performed by the action of either of the pallets *m m*, Fig. 4, the middle prong falls into the fmall notch in the roller *n*, which lies between thofe pallets; and when the unlocking is performed, and either *a* or *b* has received its tooth, the arm F renders it impoffible it fhould be accidentally difengaged, becaufe that arm is prevented from returning by the circular face of *n*, paft which it could not return at all, if the notch or excavation were not made to permit to pafs at the proper time.

The detents or locking pallets have no fpring, and are very fafe.

Laftly, as the pallet lever B always refts againft one of the teeth of the wheel A, the preffure againft the pallets *a* or *b* may be rendered extremely light, fo as to deduct fcarcely any thing from the momentum of the balance at unlocking.

Light preffure on the detent.

Fig. 3. exhibits a fimpler mode of conftruction, in which a fpring is fubftituted inftead of the pallet lever with its fpiral. The inventor remarks, that he fhould prefer this conftruction for a clock, but that the difadvantage from the weight of the fpring in different pofitions is obvious.*

Another conftruction.

III.

Remarks on the Proceffes for clarifying Liquids. By CITIZEN PARMENTIER.

(Continued from page 234.)

NEVERTHELESS, though the ufe of the filter for water intended to be drank is of high antiquity, we muft confefs that the fountains conftructed for this purpofe not only deprive them of the earth which rendered them muddy, but likewife

Filtered water is infipid, becaufe deprived of gas.

* Some account of Time-pieces and Efcapements are given in the Philofophical Journal, Quarto Series, I. 56, 429. II. 50. III. 187, 342, 416.

of

of a super-abundance of air with which they are sometimes impregnated, and gives them that lightness or brisknefs, which is found in the waters of the Seine in a greater degree than in any other known stream. The proof of this is, that by repeated filtrations we may render water flat, heavy and unwholesome.

Thus when the specific gravity of the water of the Seine is required to be determined, it is adviseable to take it from the river at the time when it is limpid, or leave it to clear by subsidence, and not to take that in preference which has been filtered; for though this operation renders waters clearer, it changes them remarkably, by depriving them, as has been observed, of the air which they contain in super-abundance.

I know a person whose palate was so accustomed to water, that he could distinguish by the taste water filtered through sand, from that which had not been so filtered. The latter appeared to him more sapid and lighter; which proves, no doubt, the privation of this air, a fact which may also be very easily ascertained under the receiver of the air-pump.

The filter cannot render salt water fresh.

Certain individuals, who are interested to maintain the contrary to what is here asserted, have affirmed, that if water were continually forced to pass through 18 feet of sand of gravel upwards, it would be purified not only of heterogeneous matters, but completely purified that is to say of its salts. This prejudice was such, that in order to support the notion the following reasoning was offered;

If these filters be sufficient to deprive water of its air, why should not the same operation be equally proper to deprive it of the salts with which it is charged; but no attention was paid to the circumstance that these salts held in solution in the water, being specifically heavier, pass along with it through the smallest apertures, whereas air being specifically lighter, and existing in a different state from that of the salts, is easily separated. And this process of application was proposed to be applied to sea water in order to render it potable. The operation has even been announced to the government as new and ingenious. It consisted in a filtration effected by force in a direction contrary to that of gravity.

But the union of saline matters with water has not a purely mechanical division. They are not interposed as some philosophers have pretended, but perfectly dissolved in water, and

 possess

poffefs the fame fluidity. Thefe falts confequently become capable of paffing through the fineft filters. No other means but evaporation is therefore adopted to feparate water from the faline fubftances which it holds in folution, and confequently diftillation alone can produce this defirable effect. But to return to the general effects of filtration:

Independently of the filters here mentioned, water is alfo clarified by means of the ftones called filtering ftones. Of thefe there are feveral kinds. They are very porous, becaufe fand enters into the greateft part of their compofition. Thefe ftones being excavated are filled with water. The fluid gradually infinuates itfelf through their pores, and appears on the external furface in the form of drops of confiderable clearnefs, which fall into a veffel placed beneath. *The filtering ftone;*

It is neceffary that thefe ftones fhould be previoufly wafhed by filtration of a large quantity of water. It is obferved, that even for feveral days the water acquires a difagreeable tafte, owing to the foreign matters diffolved by this fluid in its paffage through the ftone. Water thus clarified cannot be fit for ufe until it paffes through intirely deprived of tafte. *how managed.*

In general the filtering ftone, though highly extolled, is a bad inftrument to procure good water; for the filtration is made very flowly, and very often ftops altogether, if the inner and exterior furfaces of the ftone be not rubbed from time to time with a coarfe brufh, to detach the earth which the water depofits. To thefe inconveniences no doubt it is, that we ought principally to attribute the difufe into which this kind of filtration is fallen. *It is a bad contrivance.*

It now only remains, that we fhould fpeak of the other proceffes which are ufed to give that perfect limpidity to feveral fluids, which they can never acquire by fpontaneous clarification or by filters, whatever may be their ftructure. *Clarifying without the filter.*

Though it is true, that the remarkable opacity of fome fluids is owing to the interpofition of particles not diffolved, but merely fufpended in confequence of their extreme divifion; it is alfo certain, that under other circumftances the defect of tranfparency depends intirely upon the incomplete folubility of one or more bodies contained in thefe fluids, fo that in order to give them the defired limpidity, we muft neceffarily have recourfe to the methods which increafe the folubility of the fubftances in queftion, or effect its total feparation. *Opacity from imperfect folution;*

Albumen

removed by certain additions.

Albumen, gelatine, the acids, certain falts, lime, blood, and alcohol, may in many cafes concur to operate the clarification of certain fluids, for which the common filters would be infufficient. Neverthelefs, thefe agents cannot be indifferently ufed, and the preference given to one rather than the other always requires to be determined from a knowledge of the fluid to be clarified. Accident has fhewn for example, that two handfuls of marle reduced to coarfe powder, and thrown into the fruit in the prefs, clarifies cyder and fmall cyder.

Albumen and gelatine ufed in fining wines, &c.

The effect of the albumen and gelatine is principally feen in vinous liquors. For this reafon alfo they are ufed in fining wine, that is to fay, in producing that bright clearnefs which they can feldom acquire, and preferve by fimple repofe. In this cafe it is fufficient, that one or the other of thefe two fubftances fhould be diffolved in a fmall quantity of water, and the folution mixed with the cold wine. A fhort time afterwards a kind of net-work is feen to form itfelf through the whole fluid, and foon afterwards this net-work contracting in all directions, collects all the foreign fubftances, and carries them to the bottom.

In other inftances it is neceffary to heat the fluids with which the albumen has been mixed, and in thefe cafes the clarifying is effected at the inftant the mixture begins to boil. Moft fyrups are clarified by this procefs, and hitherto no other procefs has been difcovered to produce a better effect. It has alfo been obferved that albumen alone is not always fufficient to clarify liquids, even though they be heated fufficient to caufe them to boil; but that it is neceffary to affift its action by means of an acid, or falt with excefs of acid. In proof of this we may offer the clarification of whey as an example.

In fome inftances an acid is added.

In fact, it is proved that whey, in which albumen has been mixed, does not admit of the coagulation which carries the cheefy matter along with it, unlefs a portion of acidulous tartarite of potafh or vinegar be added at the inftant the boiling begins.

It may alfo be conceived that the quantity of acid requifite to be added in this cafe always bears relation to the ftate of the fluid, and that it would be abfurd to pretend to fix the dofe in an invariable manner.

Frefh

Fresh cream is advantageously used to clarify spirituous liquors. One or two spoonfulls for each French pint or English quart are sufficient to produce this effect, without heat, in the course of a few hours. But as in this clarification there always remains some cheesy particles suspended in the fluid, on account of their great subtilty, it is necessary to complete the process by subsequent filtration. Cream used to clarify spirits.

Lastly, there are fluids which, in order to become clear, require only to be heated nearly to the boiling point. These are principally such as owe their opacity to substances which cannot be completely dissolved, unless the temperature of the solvent be raised considerably above the natural state. Many saline solutions are in this case, and those who are busied in chemical researches have frequent opportunities of observing them. Mere heat.

Most of the juices of plants newly expressed may be partly clarified by heat. The chemist is therefore in the habit of recurring to this method with respect to the juices which, on account of their density and viscidity, are not capable of being filtered. Clarifies the juices of plants, &c.

It frequently happens that a very slight degree of heat, applied to expressed and filtered juices of certain plants, will render them at once turbid; in which case the flocculent matter that floats in the liquid settles at the bottom of the vessel. This substance was considered by Rouelle the younger as the vegeto animal matter of wheat; but I shewed, in 1772, that it is a substance similar to the white of egg: which proves that we were authorized at that period to reckon the albumen among the products of the vegetable kingdom. Vegetable albumen precipitated.

I must insist on an important observation, that in general it is necessary to separate the magma which is formed in liquors clarified in albumen, particularly when in order to concentrate these fluids it is necessary to evaporate them by boiling. Without this precaution we shall see the same magma dissolve, and the fluids become more turbid than they were before the clarification. For a like reason it is that soups, which have not been skimmed in time, always retain a cloudy and unpleasant appearance. Caution again re-dissolution of the albumen.

Though the use of albumen to clarify the juices of certain vegetables is considerable, it is not, however, exempt from inconvenience. One of these inconveniences is, that it In what cases fining with albumen is hurtful.

 changes

changes the nature of these fluids so much, that their medical properties are partly destroyed. What happens to certain pharmaceutical preparations, such as decoctions of medicines, when white of egg and heat is used to clarify them, is well known; for, in that case, they become almost ineffectual, if care has not been taken to double the proportion of ingredients which enter into their composition. Lewis has observed, that this operation deprives the syrup of diacodium of all its medical properties.

Such are the observations which I have thrown together respecting clarification. My design in communicating them has been to prove that this operation, though simple in appearance, cannot be practised without some skill and attention; and that among the number of processes commonly used, several afford results less satisfactory than others. It is necessary, therefore, to determine with regard to the choice of methods according to the nature of the substances made use of. I have confined myself to a clear narration of the facts; others may direct their attention to the explanations of the mode in which these substances operate.

IV.

On the Discovery of the Arseniates of Copper and of Iron. By a Correspondent.

Discovery of the arseniates of copper and iron attributed to Vauquelin.

AN article * of the Decade Philosophique (30 Nivose), giving an account of the first trimestre of the tenth year of the French Republic, says, that Citizen Vauquelin has enriched the catalogue of mineral bodies by the discovery of two new substances, arseniate of copper and arseniate of iron. I am far from imputing to this able analyst the desire of attributing to himself any discovery to which he has no title; but the French chemists, whether from ignorance or intention, pay so little regard to the discoveries of others, that it becomes the duty of every friend of science to point out their violations of historical justice.

True statement of their history. Klaproth.

In the year 1786, Mr. Klaproth examined an ore from Cornwall, and found it to be arseniate of copper. Schlotheion published a description of it in 1790.

* By Cit. Lacepede, Secretary.

In

In the first volume of the Annales de Chimie, page 195, there is an imperfect sketch of an analysis of arseniate of iron by Mr. Proust. Proust.

In February, 1801, the Count de Bournon presented to the Royal Society a very long and able account of a great number of varieties of these ores, which he had the good fortune to procure from Cornwall. This paper was followed by Mr. Chenevix's chemical analysis, which proved the accuracy of the Count's description. The ample account of de Bournon, and Chenevix.

The most extraordinary part of the paragraph in the French Journal is a confession, that the specimen examined by Cit. Vauquelin had been sent to the Ecole des Mines of Paris by the Count de Bournon. That gentleman never did send any until his account and Mr. Chenevix's had been made public. Cit. Vauquelin's paper must have been read at the National Institute between September 20 and December 20, 1801, six months at least after the communication to the Royal Society. Hence we see the justice of attributing that discovery to the French chemist. Extraordinary circumstances attending this *supposed* mistake.

In the Journal de Physique of Brumaire, tenth year, there is a paper of Dr. Karsten, of Berlin, upon the same subject: but which deserves no notice, further than as it regards the history of this substance. Dr. Karsten's paper.

I am, SIR,

Your humble Servant,

A CORRESPONDENT.

V.

On the Nature and Preparation of drying Oils; with a View to the Improvement of such as are used by Artists, as Vehicles for painting. By Mr.* TIMOTHY SHELDRAKE.

EXPRESSED oils, considered with a view to the painter's use of them, may be divided into two kinds; first, such as are capable of drying in some circumstances by themselves, and always with certain additions; and secondly, such as cannot be made to dry by any means whatever. Drying oils, and such as do not dry.

* Transactions of the Society for the Encouragement of Arts, for 1801, page 209.

Drying oils; linseed, nut, and poppy.

Of the firſt, which I ſhall call drying oils, there are three in common uſe, linſeed, nut, and poppy oil. The firſt is darkeſt coloured, and dries the ſooneſt; the ſecond is lighter, but does not dry ſo ſoon; and the third has leaſt colour, and dries ſlower than either of the others.

Drying oils contain mucilage.

By a proceſs, which it is perhaps needleſs to deſcribe here (*ſee Vol. xvii. page* 281),* I have ſucceeded in ſeparating from each of thoſe oils, a mucilage or gum, in a liquid ſtate, and capable of mixing with water in every proportion, though, when thoroughly dry, it would not diſſolve in cold water; but my experiments on this head were not carried to any great length. It is to be remarked that linſeed oil afforded moſt of this gum, nut oil the next largeſt quantity, and poppy oil the leaſt of all.

Olive oil contains none:

Olive oil, when treated in the ſame manner, afforded none of this mucilaginous ſubſtance; whence I was led to conclude that the eſſential difference between the drying oils, and thoſe which do not dry, conſiſts in this:—that the latter either contains no mucilage or gum, or that it is ſo intimately combined with its other principles, that it cannot be ſeparated from them in that peculiar manner which always takes place in oils which dry by themſelves, or when mixed with colours.

Drying oil affords a pellicle, which dries firſt, &c.

If drying oil is expoſed to the air, in a ſhallow veſſel, and left at reſt, a pellicle is ſoon formed on the top, and becomes externally perfectly dry. If this be removed, a ſecond will be formed in the ſame manner; and if this experiment be repeated many times on the ſame quantity of oil, without moving or ſhaking the veſſel, it will be found that the ſecond pellicle will require more time to form it than the firſt, and ſo on; till it will be found difficult to get it fairly ſkinned over in a conſiderable time. The ſame effect takes place, in a leſs viſible manner, in every quantity of drying oil which is united with colours in a picture.

From this experiment it is to be concluded, that drying oils exert that faculty by throwing up their mucilaginous parts, which become ſolid when at reſt, and in contact with the air.

* Or Philoſ. Journal, quarto, III. 345.

The

The ingredients added to oils to make them dry faster, viz. calces of lead, faline fubftances, earths or gums, are fuch as unite with and increafe the quantity of thofe parts which float to the top, and form a fkin, more or lefs dark, over the colours originally mixed with them. If we confider the nature of thefe ingredients, we fhall be at once enabled to account for a fact univerfally known, viz. that in proportion to the ftrength of the drying oil ufed in painting a picture its colour becomes depraved. It will be injured and finally deftroyed, by being kept in a damp fituation, excluded from a free circulation of air, or placed under a glafs.

When oils are rendered ficcative they deprave the colours; and how.

The defideratum is to prepare oil or other vehicle for painting, fo that the colours, when mixed with it, fhall not be debafed under any of the above-mentioned circumftances. It muft be fo prepared or ufed, that it fhall ferve as a cement to unite and bind the colours, without fkinning over them. It muft likewife not contain thofe principles which always exift in the calces of lead, faline, or earthy fubftances, which from the firft deprave the colours, and attract particles from the air, under peculiar circumftances, which increafe that depravity, till at laft the appearance of the colours is totally deftroyed.

Oils for painting ought not to fkin; nor contain changeable ingredients.

It is only among the refins or bitumens that we can expect to find a fubftance poffeffing the properties requifite to give to colours all the brilliancy and durability of which they are fufceptible. My firft attempts and experiments were made with folutions of maftic and fandarac in the painters oils; but though thefe compofitions poffeffed more brilliancy than the common drying oils, they were liable to a confiderable objection; for they did not dry readily, and when dry, were eafily acted upon by all the common folvents for refinous fubftances, and on that account muft be very deficient in durability, which is one of the moft neceffary qualities I wifhed to difcover.

Refins or bitumens anfwer this character;

Maftic and fandarac dry flowly, and are not durable.

The difficulty with which amber is in any way diffolved, fuggefted the propriety of trying that fubftance. Accordingly I diffolved it, in each of the painters oils, by Dr. Lewis's procefs, without injuring its colour; and this folution was made in the common way. It was much darker coloured in itfelf, but produced fcarcely any difference in effect when mixed with colour. By experiments with each of thefe folutions I afcertained the following facts, viz.

Amber poffeffes fuperior qualities.

Every

Experiments. Tints.

Every colour, and all the tints compounded from it, were more brilliant than correſponding tints and colours mixed with the beſt drying oils to be procured from the ſhops.

Paintings ſhut up;

Colours mixed with amber, after having been ſhut up in a drawer for ſeveral years, loſt nothing of their original brilliancy. The ſame colours tempered with oils, and excluded from the air, were ſo much altered, that they could ſcarcely be recognized.

colours expoſed to heat;

Colours tempered with amber were laid on plates of metal, and expoſed (both in the air and cloſe boxes) for a long time, to different degrees of heat, from that of the ſun in ſummer to the ſtrong heat of a ſtove, without being injured. It is needleſs to obſerve that oil colours cannot undergo the ſame trials without being deſtroyed.

and to ſolvents:

Theſe colours, when perfectly dried in any way, were not acted upon by ſpirit of wine and ſpirit of turpentine united. They were waſhed with ſpirit of ſal ammoniac, and ſolutions of pot-aſh, for a longer time than would deſtroy common oil colours, without being injured.

They dry very well.

They dry as well in damp as in dry weather, and without any ſkin upon the ſurface. They are not liable to crack, and are of a flinty hardneſs; whence it appears that this vehicle poſſeſſes every deſirable property, and it is preſumed may be a diſcovery of ſome importance to artiſts.

Gum copal nearly as good as amber, and rather brighter.

Having ſucceeded thus far with amber, I tried the ſame experiments upon ſolutions of gum copal, which is nearly as hard and inſoluble as amber itſelf. The reſult of theſe was the ſame as the former, except that with the copal the colours were ſomething brighter than with amber. As it is extremely troubleſome to diſſolve the copal and amber, I tried thoſe ſolutions of them in oil which are ſold in the ſhops. When good, I found them to anſwer as well as my own. This is a great convenience, as many might be deterred by the difficulty of preparing this vehicle, who may willingly uſe it, as it is thus to be procured without that trouble.

The Method of uſing the Solution of Amber or Copal, as a Vehicle for painting.

Inſtructions for the uſe of amber or copal as a vehicle for painting.

The cloth or other ſubſtance to be painted on, ſhould be prepared with ſome colour fully ſaturated with drying oil; or it will be better done with the ſame vehicle to be uſed in painting,

painting. If it is not fully ſaturated, it will abſorb ſome of the vehicle from the colours, which is commonly termed the colours ſinking in.

Grinding colours, &c.

All the colours which require grinding, ſhould be previouſly ground in ſpirit of turpentine. All the pure parts ſhould be tempered with ſuch a quantity of the vehicle as will enable them to lie on the pallet. The white ſhould be tempered as ſtiff as poſſible. All the tints ſhould be made by mixing the colours ſo prepared without any more of the vehicle, but they ſhould be diluted with ſpirit of turpentine, if neceſſary for working.

Conſequences or effects.

If the ground is properly prepared, and the above caution obſerved in tempering the colours, it will be found that all the dark colours in the picture will bear their full tone, and have a demi-tranſparency, which increaſes their native brilliancy, without the dingy appearance ſo common in ordinary oil-painting. The admixture of white increaſes the body of the colours progreſſively, till there will be left in the lighteſt parts, only ſo much of the vehicle as will bind the colours, and give them their full tone, but with very little of a ſhining appearance. When the picture is perfectly dry, it ſhould be varniſhed with a maſtic or ſimilar varniſh. Perhaps the beſt would be copal varniſh made by ſolution in ſpirit of turpentine, or ſpirit of wine.

Obſervations.

The rationale of this vehicle ſeems to be this: the amber and copal, when diſſolved in oil, form a homogeneous maſs, which dries by inſpiſſating, inſtead of ſkinning over, like the common drying oils, which conſiſt of heteregeneous parts, ſome of which ſeparate and dry on the top.

As the amber and copal are not ſoluble in any of the menſtruums which diſſolve moſt reſinous ſubſtances, pictures painted with them cannot be injured, if cleanſed with thoſe menſtruums: and as they are extremely hard, and the moſt durable ſubſtances of their claſs, they protect the colours from every kind of injury, more effectually than any other known vehicle.

Conjectures to ſhow that the foregoing vehicles were uſed by the older painters.

No. III. *Conjectures tending to ſhow that the Vehicle which I have deſcribed, is ſimilar in Principle, if not identically the ſame, as that uſed by ſeveral of the older Painters, who were eminent for their Skill in colouring.*

Lomazzo.

Lomazzo, an eminent painter, and pupil of Leonardo da Vinci, publiſhed a Treatiſe on Painting, in which it is mentioned that linſeed or nut oil was generally uſed for painting: he likewiſe obſerves, that powdered glaſs was uſed as a dryer. As Lomazzo was blind when he publiſhed his treatiſe, he could have no motive for keeping any thing which he knew ſecret; whence it is to be concluded, that thoſe oils were generally uſed for painting in his time, and that he knew of no exceptions to the practice.

Leonardo da Vinci.

In one part of L. da Vinci's Treatiſe on Painting, he mentions *nut-oil and amber*. As we know that amber gives peculiar brilliancy to colours, that L. da Vinci was peculiarly celebrated for the richneſs of his colouring, and are informed from his own writings that he was acquainted with ſolution of amber in nut-oil, it is to be preſumed *that* was the vehicle he uſed. If this ſuppoſition is not to be admitted, we muſt believe that he knew how to diſſolve amber in nut-oil (a proceſs at that time both tedious and troubleſome), without knowing the beſt uſe to which he could poſſibly apply it.

Leonardo's biographer ſays, "When he was at Rome, Leo X. reſolved to employ him. Leonardo hereupon ſets himſelf to the diſtilling of oils, and the preparing of varniſhes to cover his paintings withal: of which the Pope being informed, ſaid, pertly enough, that he could expect nothing of a man who thought of finiſhing his works before he had begun them. Leonardo therefore left Rome without having been employed."

I muſt beg leave to diſſent from his Holineſs's opinion. If my idea of Leonardo's vehicle be juſt, it was natural for him to begin the preparation of it as ſoon as he knew that he was to be employed as a painter: and as the ſpirit of that time led every one who made any uſeful diſcovery, to preſerve it as a valuable ſecret, it was equally natural for him to account for his employment by ſaying that he was preparing varniſhes. Whatever his ſecrets were, they remained unknown to the world till 1651, when his Treatiſe on Painting was publiſhed.

The

The next intimation of ſolutions of amber which I have obtained is from the works of Boyle, who gained much of his information from Italian chemiſts; whence it is evident that the knowledge of this preparation is of long ſtanding in that country; and its uſe, if it was uſed at all in the arts, is to be ſought for in the works of Italian artiſts. Robert Boyle.

Whoever examines the Venetian pictures with attention, conſiders that the beſt artiſts of that ſchool were remarkable for the facility with which they worked, and reflects on ſome paſſages in Lomazzo, will be diſpoſed to admit that the peculiar ſkill of the Venetian painters depended on three circumſtances, viz. the colours they uſed, their method of uſing them, and the vehicle they worked with. Of the firſt, Lomazzo gives poſitive information: the ſecond can never be known without information equally poſitive; but of the vehicle ſome knowledge may be obtained by way of analyſis. Till that knowledge is obtained, I may perhaps be excuſed for hazarding the following conjectures. On the pictures of the Venetian ſchool.

If my experiments have not miſled me, I am entitled to draw the following concluſions from them. Wherever a picture is found poſſeſſing evidently ſuperior brilliancy of colour independent of what is produced by the painter's ſkill in colouring, that brilliancy is derived from the admixture of ſome reſinous ſubſtance in the vehicle. If it does not yield on the application of ſpirit of turpentine and ſpirit of wine ſeparately or together, or to ſuch alkalies as are known to diſſolve oils in the ſame time, it is to be preſumed that vehicle contains amber or copal, becauſe they are the only ſubſtances known to reſiſt thoſe menſtrua. Superior brilliancy of colour from the vehicle,

I have been told, and ſome experiments of my own prove the information to be true, that the Venetian pictures, conſidered with reſpect to vehicle, are of two kinds: for ſome are extremely hard, and not at all affected by any of the above menſtrua; others are ſimilar in colour, but ſo tender that it is ſcarcely poſſible to clean them without injury, and in that reſpect are little ſuperior to mere turpentine colours. The firſt, in conſequence of the data which I have laid down, incur the ſuſpicion of being painted with amber or copal, but how are we to diſtinguiſh with which? which appears to have been amber or copal.

As each of theſe ſubſtances reſiſts equally the common menſtrua, perhaps the diſtinction can only be made by aſcertaining Attempt to make the diſtinction between them.

taining the date of the picture. For example:—if it is found to have been painted before copal was known in commerce, it may safely be said to have amber for its basis; but if it has been painted after that period, I know of no method of distinguishing which of the two was made use of. As copal could not have been known, as an article of trade, before the seventeenth century, it follows that all pictures painted before that period, and possessing the properties I have described, must have amber for the basis of their vehicle. As this exception necessarily includes all the Venetian artists of the first class, we are therefore authorised to conclude that, if the works of these artists can bear the test of the menstrua I have mentioned, amber was the basis of the vehicle with which they were painted.

Ancient recipe, in which turpentine was the solvent of copal.

I once saw a recipe for dissolving copal, said to have been brought from Venice towards the close of the last century. The process was, to melt Venice turpentine upon the fire, to add gradually copal powdered, stirring them together to be united in fusion, and afterwards spirit of turpentine, in order to dilute it to the consistence of varnish. I tried this process, but it did not succeed.

It was not common, but Chio turpentine.

Upon inquiry I found that the Venice turpentine of the ships was only common resin, dissolved in spirit of turpentine to a proper consistence; whence the cause of my failure was evident. Reflecting on the commercial pursuits of the Venetians in the fifteenth and sixteenth centuries, I was led to conjecture that the substance called originally Venice turpentine was the product of some country intimately connected with them. Pursuing this idea, I procured, with much difficulty, some Chio turpentine, repeated my experiment, and succeeded completely. Besides the property of uniting easily with copal, it had others that excited my attention. Common resins, if exposed to fire, burn with extreme fierceness and rapidity; but when some of this was laid on the point of a knife, and held in the flame of a candle, it melted and dropped down before it began to burn. It emitted a peculiarly grateful smell; was of a most beautiful pale gold colour; was more brilliant than any turpentine I had ever seen; and when diluted to the consistence of varnish, perfectly resembled in colour, a solution of copal which I made in spirit of turpentine with camphor.

This turpentine differs from the common sort.

I showed

I ſhowed ſome of this to a gentleman who was converſant in ſuch ſubjects. He told me that, when at Venice, he frequently rubbed pictures violently with his handkerchief, to try if he could diſcover what they were painted with; and when ſo rubbed, they ſmelt exactly like what I then produced to him.

Additional probability that the Venetians uſed it.

As I had previouſly perfected what I thought to be a ſuperior vehicle, with which this could not vie in hardneſs and durability, I did not proſecute my experiments with this any farther; but as it unites rapidly with copal, and poſſeſſes all its viſible properties, I may be permitted to conjecture that it would have ſimilar effects when mixed with colours: and if there was any ſecond, inferior, and common vehicle, ſimilar in its viſible properties to the laſt, and ſo much within the reach of the moſt ordinary painters, as to give their works one common mark with thoſe of the firſt artiſts, it would be difficult to point out a ſubſtance more likely to afford it than this which muſt have been common in their own country, ſince its name is ſtill attached to ſubſtances of the ſame claſs throughout Europe, though its real properties are now but little known.

This laſt compound leſs hard and durable than the former.

If this was the baſis of the common Venetian vehicle, it might have been uſed with or without oil. If the latter, the works of the common Venetian painters muſt have been mere varniſh painting: if the former, it muſt have been compounded with the oil, according to the principles I have already explained. I am inclined towards the latter opinion, from having heard an obſervation attributed to Bombelli, a celebrated Venetian painter, who ſaid, "*That he wiſhed his pictures to dry as faſt as poſſible, that the oil in them might not riſe to the ſurface, and turn yellow.*"

It might be uſed with oil or without.

To this conjecture it may be objected that turpentines and compounds from them do not dry well. I am not prepared to anſwer this objection, as I have made no experiments relative to it; but it certainly is not concluſive, as ſuch compounds may not dry well in this country, though they may in the warm climate of Italy.

In the *Maniere d'imprimer les Tableaux*, publiſhed by Le Blond at Paris, 1740, is a recipe for the varniſh he uſed on the coloured prints executed by him, in this country, before he

Varniſh of Le Blond.

he went to France. It is as follows:—"Take four parts of balſam of capivi and one of copal. Powder and ſift the copal; and throw it by degrees into the balſam of capivi, ſtirring it well each time it is put in: I ſay each time; for the powdered copal muſt be put in by degrees, day after day, in at leaſt, fifteen different parts. The veſſel muſt be cloſe ſtopped, and expoſed to the heat of the ſun, or a ſimilar degree of heat, during the whole time; and when the whole is reduced uniformly to the conſiſtence of honey, add a quantity of warm turpentine; *Chio turpentine is the beſt.*"

its durability and excellence.

Le Blond's prints were long neglected, and are now forgotten. Whatever difference of opinion may prevail reſpecting them, there can be none reſpecting his varniſh, as I have ſeen ſome of theſe prints in perfect condition, notwithſtanding they had been thrown careleſsly about for nearly ſixty years.

His intelligence probably from Italy.

Le Blond was a pupil of Carlo Maratti. He died at a very advanced age, leaving behind him the character of an ingenious projector. It is probable that he might collect much information analogous to his purſuits during a long life; but it is more probable that he obtained much of it where he received his education. Thus, wherever we find notices of the uſe of theſe ſubſtances in the arts, they invariably lead us towards Italy, where they certainly were firſt known.

I have thus detailed the circumſtances which impreſs me with a conviction that the vehicle I have offered to public notice is, in ſubſtance, the ſame as that uſed by the beſt colouriſts of the Italian ſchools. What impreſſion the facts I have enumerated may make upon others I know not: but ſtill the truth of my opinion muſt be determined by experience; for it would be of ſmall conſequence to prove that this vehicle was uſed in former times, unleſs it can likewiſe be made evident that it will be uſeful to the preſent race of artiſts.

Yours, &c.

TIMOTHY SHELDRAKE.

Strand, February, 1801.

VI.—On

VI.

On the Native and Artificial Sulphurets of Iron, by PROFESSOR PROUST *.

Why native fulphurets of iron are not foluble in acids like the artificial.

IN a former memoir † I remarked, that the reafon why the acids which eafily diffolve the artificial fulphuret, do not exercife the fame power on the native fulphuret, confifts in an excefs of fulphur in the latter, which art has not yet fucceeded in combining with iron. Hitherto in fact, I have not prefumed that our imperfect means could approach fufficiently near thofe of nature for us to hope to imitate the formation of pyrites. Accident, however, has lately removed this difficulty.

Iron pyrites formed by art.

I heated, without any particular attention to the quantities, a mixture of about ten ounces of fulphur and of filings, in order to fupply my laboratory; and judging by the colour that the quantity of fulphur was infufficient, I thought proper to add a new dofe. In confequence of this, the crucible was made nearly red hot; but not to fufion, becaufe it is more convenient for ufe to obtain it in the ftate of powder. When I afterwards tried to diffolve it in an acid properly diluted, I was fomewhat furprized to find that it did not afford fulphurated hidrogen. It was to no purpofe that I altered the ftrength of my acid, for I obtained no gas. This unexpected refult fhewed the poffibility of forming pyrites by art.

Roafted pyrites reftored by giving it the requifite dofe of fulphur.

Since pyrites becomes foluble as we have feen, only by depriving it of the fulphur which exceeds its point of faturation, it appeared to me, that I fhould firft endeavour to reftore all its qualities by producing its firft ftate from a fimilar excefs of fulphur; and this in fact fucceeded.

Experiments.

I mixed an indeterminate quantity of fulphur with four hundred grains of the pyrites of Soria, deprived of its excefs by diftillation, and I heated the mixture in a retort befide another, which alfo contained four hundred grains of pyrites in the crude ftate. My object in this laft arrangement was to obtain a kind of thermometer, to prevent my giving too great a degree of heat to the firft retort; that is to fay, not to ex-

* Journal de Phyfique, Pluviofe X year.

† Philof. Journal, I. 109.

pofe

pofe this fecond dofe of fulphur, which a new attraction was to add, to that temperature which would conftitute the firft point of faturation with regard to iron.

At a given heat the fuper-abundance of fulphur was carried off by diftillation, after which the two retorts being kept an hour longer at the fame temperature, did not exhibit the flighteft vapour of fulphur.

Regenerated pyrites.

The regenerated pyrites was pulverulent; a proof that it had retained no portion of fulphur beyond the point of faturation, otherwife I fhould have found it in a pafty ftate, having the figure of the retort.

Characters.

It had refumed the greenifh yellow colour, which is that of crude pyrites when pounded, whereas its tinge was before dull and blackifh like fulphurated iron, which is capable of affording hidrogen. Its weight was found to be five hundred and four grains, that is to fay, the diftilled pyrites had refumed twenty-fix pounds of fulphur per quintal.

Proportions of parts.

Obfervations on the quantities; to prove that the artificial pyrites is the fame as the native.

By infpecting the former memoir, we fee that the mean product of two diftillations, each confifting of four hundred grains of pyrites, was three hundred and eighteen for the refidue, and feventy-eight for the fulphur, to which muft be added three or four for what was carried away by the gas, fo that in the quintal we have feventy-nine and a half refidue, and twenty and a half fulphur. According to this report, four hundred grains of the refidue, or diftilled pyrites, ought to have taken only ninety-eight and a fraction of fulphur, whereas the refult of our experiment gives one hundred and four. The difference being no more than one and half per quintal, may be afcribed to the inaccuracy of experiment, as well as the nature of the pyrites, which is not an homogeneous combination; for befides the clay and the fand, it often contains a fmall portion of oxide of iron, which may have caufed the fixation of a fomewhat greater quantity of fulphur than the lofs it fuffered by diftillation.

Trial of the regenerated pyrites by acids; which did not diffolve it.

I afterwards examined my regenerated pyrites with a fulphuric acid of ten degrees by the areometer of Baumé, which acid diffolves the diftilled pyrites very well; but I obtained only a few ounces of fulphurated hidrogen. I afterwards heated the mixture; it afforded a flight portion of gas, after which the pyrites remained without alteration. Long continued boiling was ineffectual for faturating my acid.

With

With the muriatic acid I fucceeded in forming a fmall portion of gas; but its action foon ceafed, and it was equally impoffible for me to faturate the acid. It was of twelve degrees of the areometer, and the powder preferved its colour, The natural pyrites treated in the fame manner does not afford the flighteft fufpicion of gas; but we muft not overlook that art cannot give to its compounds that aggregate condenfation which is one of the greateft obftacles to folution. Morveau and Fourcroy have given us very ftriking proofs in the refiftance they found in diffolving the native oxides of iron and tin.

The aggregation of native pyrites in fome meafure defends it.

The pyrites not being, as I have obferved, an homogeneous combination, it is evident that we could not expect to difcover the true proportions of fulphur wherein iron could attract by thefe previous experiments; for which reafon I made the following.

Thefe experiments could not fhew the proportions of fulphur.

In order to depend in the firft place upon pure filings, we muft begin by heating them gently, and for a confiderable time in a glafs retort. Under thefe circumftances, it is not a little remarkable to obferve, that filings cleaned with the magnet, and kept in well-clofed bottles, afford neverthelefs a very ammoniacal water, and even muriate, if I am not deceived in the tafte of the fluid which efcapes.

Other experiments.

Iron filings afford ammonia.

One hundred parts of filings heated to a low red heat in a retort, and fulphur being let fall upon them, acquire an incandefcence which was noticed even by the ancient chemifts; but they do not become faturated. The increafe is found to be only twenty or thirty parts. After pounding the product, then mixing it with fulphur, and expofing it to a red heat, a refult is obtained, which weighs pretty conftantly 159; but which, I think, may be fet at 160, on account of the impurities of the iron.

Direct combination of iron and fulphur.

Firft degree of fulphurization: 60 parts of fulphur to 100 iron.

The product is iron fulphurated to the firft degree. It may be fufed, and even melts in the retort, if harpfichord wire be ufed, and maintains itfelf without alteration. Its colour is metallic, but dull and confiderably different from the golden colour of the pyrites. In a word, it is the fulphuret which is proper to afford fulphurated hidrogen gas.

Its appearance.

In order to difcover the excefs which this fulphuret can ftill abforb, provided its temperature be lefs elevated, I treated two hundred grains of filings with the before-mentioned

Second degree, 89 fulphur to 100 iron.

tioned precautions, and obtained three hundred and eighteen grains of ſulphuret. When taken out of the retort it was mixed with a new doſe of ſulphur, and heated beſide another which contained crude pyrites. The ſuper-abundant ſulphur paſſed in diſtillation, and the retorts were kept for an hour at the ſame heat. The reſult of this operation was an artificial pyrites, weighing three hundred and ſeventy-eight grains. It was pulverulent, which proved that it had retained no more ſulphur than its attraction could take up.

Its appearance and habitudes.

Its colour was no longer blackiſh, but greeniſh yellow. When treated with the acids, its habitudes were preciſely the ſame as thoſe of the regenerated pyrites; and laſtly, it differed from the native pyrites only by the want of that denſity which the latter received from their humid cryſtallization.

Conſequences.

Recapitulation of the facts, with obſervations.

From the foregoing facts it follows, that iron can fix ſixty per cent. of ſulphur by a conſiderably elevated temperature. This proportion conſtitutes iron ſulphurated to the minimum.

Sulphuret at the minimum 10 ſulph. + 6 iron.

By a lower heat it can alſo attract a quantity which is equal to the half of this weight; and this reſult is iron ſulphurated to the maximum, or with ninety parts of ſulphur. If this laſt combination be expoſed to the temperature which formed the firſt, it returns to that ſtate; that is to ſay, it returns to the minimum of ſulphuration, by giving out all the ſulphur it was capable of fixing above the proportion of ſixty parts for each quintal of iron. Iron ſulphurated to the maximum is the pyrites, and poſſeſſes all its properties excepting denſity.

Sulphuret at the maximum 10 ſulph. + 9 iron.

Pyrites reduced to the minimum by heat with iron filings,

In order to render the highly-ſulphurated iron fit for affording hidrogen, it need only be heated with half its weight of filings.

or by diſtillation,

To apply pyrites to the ſame uſe, the ſame treatment may be adopted, or they may be diſtilled, in order to deprive them of that portion of ſulphur which conſtitutes the difference between the ſulphuret at the minimum, and that at the maximum.

not found native.

The mineral kingdom has not yet preſented iron ſulphurated to the minimum. In the yellow coppery pyrites, which is a compound of two ſulphurets, the iron is always at the maximum of its ſaturation, and accordingly theſe ores reſiſt all acids, except ſuch as can oxide the exceſs of ſulphur.

Copper in both ſtates?

I ſhewed in my former memoir, that the native ſulphuret of copper is uſually found with an exceſs of ſulphur amount-

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ing to 14 or 15 per cent; it is very poffible, that this metal may in its faturation follow the fame law as iron, which requires to be examined; but only in the fulphurets which are pure; for in thofe which are complicated, or thofe in which the fulphuret of copper is an integrant part, I have found it without excefs of fulphur.

Copper pyrites, are mixed: their habitudes.

The yellow copper ores, or the native union of the two fulphurets of copper and iron fulphurated to the maximum, afford by diftillation lefs fulphur than the fimple pyrites, becaufe the fulphuret of copper in this mixture has no excefs. The beautiful copper pyrites of Avalar in Bifcay, affords no more than one twelfth of fulphur by diftillation.

Analyfis of the compound pyrites.

If thefe pyrites be fufed with a portion of potafh, the excefs of fulphur unites to this alkali, and the fulphuret of iron is reduced to the minimum. The aqueous fulphuric acid may then be ufed to analyfe them. It diffolves all the fulphuret of iron without affecting that of copper, which in that cafe prefents itfelf with a deep blue colour, which is one of its characters. By this means we may obtain the proportion of the two fulphurets which compofe this kind of mineral. But when we know the quantity of copper contained in a copper pyrites, we always know that of the fulphuret of the metal, becaufe in nature, as well as in art, copper never takes up more or lefs than 28 grains of fulphur per quintal.

Copper in nature and alfo by art takes up exactly 28 per cent. of fulphur.

If we wifh to afcertain the quantity of fulphuret in a copper pyrites, its nitric folution muft be precipitated by fulphurated hidrogen, and the precipitate ignited in a retort. The product always reprefents the real quantity of fulphuret originally combined in the mineral.

Native Sulphuret of Manganefe.

Native fulphuret of manganefe.

This fulphuret has not been noticed by mineralogifts as far as I know. I difcovered it fome time ago in certain fpecimens of gold-ore from Nagyag.

Hiftory and defcription.

The gangue of the piece which afforded this fulphuret is a carbonate of manganefe mixed with quartz, like that of the fulphuret of Tellurium. It does not prefent metallic cryftals of this metal, but a multitude of points, which under the magnifier appear to be an affemblage of pyritous parts. This mineral treated with the aqueous fulphuric acid, affords a

Treatment.

very abundant mixture of carbonic gas and fulphureous hidrogen. The firft arifes from the decompofition of the carbonate, and the fecond from that of the fulphuret.

This new fulphuret of manganefe very eafily known.

It is extremely eafy to afcertain the exiftence of this new fulphuret. Firft, becaufe among all thofe we are acquainted with there is none which is fo eafily decompofed, and with fuch abundance of this gas. Secondly, becaufe the artificial fulphuret of manganefe obeys the action of this acid with the fame celerity. And thirdly, becaufe we do not difcover any thing elfe in the folution but oxide of manganefe, and a very minute quantity of that of iron. The fpecimen does not contain either gold, tellurium, lead, or any other metal.

Ore of tellurium.

In the true ore of tellurium, I found that the fulphurets of lead and of tellurium are combined together, and that the gold is native, and not at all mineralized.

Is the manganefe, metallic, or oxided?

I fhall add nothing further refpecting the fpecies of fulphuret here mentioned, becaufe I have not enough to decide whether the manganefe exifts in the ftate of oxide or metal. If it be an oxide, its fulphuret may probably derive from a very condenfed aggregation the power of eluding the activity of the atmofpheric oxigen; for the artificial fulphuret paffes with confiderable fpeed to the ftate of black oxide mixed with fulphate.

Concerning the Dif-oxidation of Iron.

On difoxidation.

When we confider the facility, or low temperature which feveral oxides require to return to the metallic ftate, we cannot help thinking, that thofe which refift the effort of our furnaces, would alfo become difoxided, if it were poffible for us to heat them to the neceffary degree.

Incident refpecting the reduction of iron by mere heat.

In order that iron may no longer remain in the clafs of oxides which are incapable of reduction without the affiftance of charcoal, I think it proper to record in this place an experiment, for which I am indebted to the friendfhip of Naft, manufacturer of porcelain, at that time eftablifhed in the Fauxbourg St. Antoine.

A bar of iron oxided nearly all through was reduced by heat in a porcelaine furnace.

The mouth of his furnace for fecond firing, which is above the furnace itfelf, was fupported by a bar of iron about an inch in diameter. The oxidation had approached fo near its centre, that being no longer fupported but by a thread of iron of

of one line in diameter, it fell and broke to pieces. I collected the fragments, and feparated the metallic thread by a flight blow. The oxide differed in no refpect from the fcales, which are feparated from iron when forged.

I placed eight ounces of this oxide in a porcelain crucible inclofed in one of the faggers below. I was not ignorant that nothing more is neceffary, than the fall of a fingle nail to deftroy a whole pile. The following was the refult: we found the crucible and the fagger perforated; the iron reduced and well fufed had bedded itfelf in the floor of the furnace. We could not feparate it but by blows of the chiffel. It was not brittle. Did this iron melt merely by the intenfity of the fire, or did it oxigenate fome charcoal, or decompofe the calcareous part of the pottery, &c. It would I think be an interefting object to afcertain this. I kept this iron for a long time, and do not at prefent know what is become of it. I was not fufficiently aware at the time how much the theory of chemiftry might be interefted in its examination. It is much to be defired that the experiment fhould be repeated.

Obfervations and queries.

Concerning Wax.

I think I may announce to you, that wax exifts in the green fecula; I had before found it in that of opium, and expect to find it ready formed in the fecundating powder. I fhall at fome future opportunity treat it in the fame manner as the fecula.

Wax in the green fecula of vegetables.

VII.

General Obfervations on the Caufes which influence the Weather in England, and the popular Methods of judging of the Weather. By James Capper, *Efq. formerly Colonel and Comptroller General of the Army and Fortification Accounts on the Coaft of Coromandel*.*

THOSE who are furnifhed with proper inftruments, and who carefully obferve the information they afford, will not often be miftaken in their judgment of the changes of the weather.

Philofophical inftruments are of great utility for judging of the weather;

* From his "Obfervations on the Winds and Monfoons" 4to, London, 1801.

weather. The barometer, the thermometer, the hygrometer, and the electrometer, will generally give us timely notice of any material changes in the ſtate of the atmoſphere. But before we conſider the beſt, or at leaſt the uſual modes of employing theſe inſtruments, we will beg leave to mention ſome common remarks of the peaſantry, whoſe profeſſions requiring them to live much in the open air, their opinions merit very great attention, being the reſult of local obſervation, continued from father to ſon, and verified from the experience of many ages. Amongſt the firſt of theſe is one, now eſtabliſhed into a proverb, that a rainbow in the morning is the ſhepherd's warning, but a rainbow at night is the ſhepherd's delight.

yet the common remarks of peaſants are valuable.

Morning rainbow denotes rain; evening rainbow, fair weather.

In a country with the ſea or ocean to the weſtward, and the wind from the ſame quarter, this opinion is likely to be true. If therefore, the clouds to the weſtward in the morning are ſaturated with moiſture, which they muſt be to produce a rainbow, as theſe clouds proceed from the W. towards the E. they probably will produce rain; whereas, on the contrary, when the ſun ſets perfectly clear, and the clouds to the eaſtward are moiſt, it is a proof that the wet clouds are paſt, with a weſterly wind, and the ſhepherd therefore may reaſonably expect fine weather on the following day.

Explanation of the prognoſtic. With a weſt wind the morning rainbow to the weſt denotes coming ſhowers; evening rainbow ſhowers paſt.

When it rains with an eaſt wind, it probably will rain for twenty-four hours. This is another obſervation, which ſeems to me applicable to countries ſituated as above-mentioned, with land to the eaſtward; for, in general, the weather is dry in theſe countries with an eaſt wind, but when the coheſion of the air and water is broken, the rain will not be violent, but of long duration.

Rain, if from the eaſt, laſts 24 hours.

Explanation.

The weather generally clears at noon, but when it rains at mid-day, it ſeldom clears up again till ſun-ſet. The air, when dry and warm, continues to abſorb and retain the moiſture continually evaporated from the earth; as therefore the ſun advances towards the meridian, and for an hour or two afterwards, he dries and warms the air; and conſequently the rain is likely to ceaſe at that time. But if there ſhould be ſo much water in ſolution in the atmoſphere, that the heat of the ſun is not ſufficient to produce theſe effects, in that caſe the rain will probably continue ſome hours longer.

Weather uſually clears at noon, but if not, then at ſun-ſet.

Explanation.

Violent

Violent winds generally abate towards ſun-ſet.

Strong winds abate at ſun-ſet becauſe the change of temperature changes the cauſe.

If we admit that wind is only a current of air put in motion by the rarefaction of the atmoſphere in ſome particular place, and that this current of air is moving towards the point of rarefaction to reſtore the equilibrium, we muſt ſuppoſe, that as the ſun declines the rarefaction will diminiſh, and conſequently the velocity of the wind decreaſe. But this obſervation, in my opinion, rather applies to the temperate than to the torrid zone; for in whirlwinds and hurricanes the contrary may very often occur.

Weather ſteady when the wind follows the ſun.

When the wind follows the courſe of the ſun, it is generally attended with fair weather. This frequent and regular change of wind, which is never more than a moderate breeze, proves that there is no point of conſiderable rarefaction near, and therefore, the current of air follows immediately the ſun's courſe: it always happens in ſummer, but very ſeldom when the ſun's meridian altitude is leſs than 40 degrees.

A ſtorm follows a calm; becauſe the air over any ſpot is more heated when quieſcent, and the ſurrounding air ruſhes in.

The changes which take place in the atmoſphere are principally marked by the riſing and falling of the barometer, which apparently is cauſed by heat and cold, the hands with which nature performs her meteorological operations: by the former the atmoſphere is rarefied, and conſequently becomes light; by the latter it is condenſed, and conſequently becomes heavy. Hence probably the old remark, that a ſtorm generally follows a calm; for during a calm the air is rarefied and expanded, and the cold air will ruſh forward in a ſtrong current to reſtore the equilibrium, and neceſſarily produce what is generally called a gale of wind, the violence of which alſo will of courſe be in proportion to the degree of its preceding rarefaction.

The barometer confirms the fact.

For theſe reaſons, the barometer falls ſuddenly whilſt the air is expanded before a gale of wind, and riſes again gradually as the condenſed air returns, and the gale in like manner by degrees ſubſides.

Summer ſhowers denoted by a fall of the barometer.

It muſt however be obſerved, that an extraordinary fall of the mercury will ſometimes take place in ſummer, previous to heavy ſhowers of rain, particularly if attended with thunder and lightning; but in ſpring, autumn, and winter, the ſudden extraordinary deſcent of the barometer indicates principally violent wind.

Upon

Why the barometer varies little near the equator, and very much in high latitudes.

St. Helena.

Madras.

S. of Europe.

England.

Peterſburgh.

Upon theſe principles likewiſe we may account for the riſe and fall of the barometer in the different zones. In the torrid zone, particularly at St. Helena, and the iſlands of the Pacific ocean, it ſeldom varies more than three tenths; at Madras about five-tenths; in the ſouth of Europe not more than one inch and two-tenths; in England it varies two inches and a half, and in Peterſburgh three inches four-tenths. In the two firſt, the temperature of the atmoſphere is not ſubject to much variation, and never to any great degree of condenſation. In the third, reckoning from the tropics to the latitude of 40°, the atmoſphere may ſometimes be ſuddenly condenſed by currents of cold air from the N. and ſtill more ſo in England. But the greateſt variation muſt neceſſarily take place on the continent to the northward, where, during the ſummer, the weather is as hot as within the tropics; and, in winter, the thermometer for many weeks, continues ſeveral degrees below the freezing point.

Thermometer; hygrometer; electrometer.

The thermometer alſo, which meaſures the degree of heat in the air near the earth, will contribute towards denoting when changes are likely to take place in the lower regions of the atmoſphere; the hygrometer diſtinguiſhes the quantity of moiſture in the atmoſphere, and the electrometer will point out the quantity of electricity which prevails in it.

The barometer little eſteemed by farmers, becauſe the words engraved thereon are fallacious.

The words generally engraven on the plates of the barometer rather ſerve to miſlead than to inform; for the changes of the weather depend rather on the riſing and falling of the mercury, than on its ſtanding at any particular height. When the mercury is as high as fair, or at 30 degrees, and the ſurface of it is concave, beginning to deſcend, it very often rains; and on the contrary, when even the mercury is at 29 degrees, oppoſite to rain, when the ſurface of it is convex, beginning to riſe, fair weather may be expected. Theſe circumſtances not being known, or not duly attended to, is the principal cauſe that farmers and others have not a proper confidence in this inſtrument.

Daily change in the barometer, and why.

It muſt alſo be obſerved, that *cæteris paribus*, the mercury is higher in cold than in warm weather, and commonly early in the morning, or late in the evening, than at noon, which ſeems occaſioned by the obvious cauſes of the atmoſphere being condenſed by the cold of the night, and rarefied by the heat of the day.

The

The following obſervations of Mr. Patrick ſeem confirmed by experience.

The old obſervations of Patrick are good.

1. The riſing of the mercury preſages, in general, fair weather, and its falling foul weather, as rain, ſnow, high winds, and ſtorms.

2. In very hot weather the fall of the mercury indicates thunder.

3. In winter the riſing preſages froſt; and in froſty weather, if the mercury falls three or four diviſions, there will certainly follow a thaw; but in a continued froſt, if the mercury riſes, it will certainly ſnow.

4. When foul weather happens ſoon after the falling of the mercury, expect but little of it, and, on the contrary, expect but little fair weather, when it proves fair ſhortly after the mercury has riſen.

5. In foul weather, when the mercury riſes much and high, and ſo continues for two or three days before the foul weather is quite over, then expect a continuance of fair weather to follow.

6. In fair weather when the mercury falls much and low, and thus continues for two or three days before the rain comes, then expect a great deal of wet, and probably high winds.

7. The unſettled motion of the mercury denotes uncertain and changeable weather.

But to theſe remarks may be added, that, when the barometer ſuddenly falls two or three tenths, without any material alteration in the thermometer, and the hygrometer is not much turned towards moiſt, a violent gale of wind may be expected. When the hygrometer inclines far towards moiſt, with only a trifling deſcent in the barometer, it denotes a paſſing ſhower and little wind; and when the barometer falls conſiderably, and the hygrometer turns much towards moiſt, the thermometer remaining ſtationary, and rather inclined to riſe than fall, both violent wind and rain are likely to follow in the courſe of a few hours.

Prognoſtics of violent wind; of ſhowers; or of a ſtrong gale of wind and heavy rain.

General or common Prognoſtics of the Weather.

Common prognoſtics.

Amongſt theſe we may reckon ſuch as are derived from birds, beaſts, inſects, reptiles and plants, to which might be added great part of the wood work in houſes, as doors, windows, window ſhutters, &c.

Birds

Birds drefs their feathers againft rain,

Birds in general retain in the quill part of their feathers a quantity of oil, which when they feel an extraordinary degree of moifture in the atmofphere, they exprefs, by means of their bills, and diftribute it over their feathers, to fecure their bodies againft the effects of an approaching fhower.

and fwallows fly low.

Swallows in purfuit of the flies and infects, on which they prey, keep near the earth in wet weather; and in dry weather, from the fame caufe, they fly much higher.

Cattle feed haftily, and go to fhelter.

Domeftic animals, as cows and fheep, but particularly the latter, on the approach of rain, feed with great avidity in the open field, and retire near the trees and hedges as foon as they are fatisfied. In fine weather they graze and lounge about, eating and refting alternately with apparent indifference.

The pimpernel and dandelion much affected by the approach of wet weather.

The pimpernel, commonly called peep-a-day, or fhepherd's weather-glafs, clofes its leaves before rain; and the down of the dandelion is much affected by moifture.

Wood work fwells.

All wood, even the hardeft and moft folid, fwells in moift weather. The vapours infinuate themfelves into the pores of trees, and alfo into the wood-work of houfes.

Infects alter their conduct in various ways.

Infects and reptiles of all kinds feek or avoid rain according to their refpective habits, by thefe means giving notice of every change of weather.

The drains emit a fmell;

because the air does not abforb the vapors.

On this fubject fee Philos. Journal, quarto, IV. 135.

It is a well known fact, that before rain, particularly in fummer, a ftrong fmell is perceived from drains and common fewers, as well as from every other body emitting a great quantity of effluvia. During fair weather, even in the fummer, the atmofphere readily abforbs all the vapours and exhalations from the earth until it is completely faturated, and confequently the effluvia from the bodies which emit them, will then be confined and afcend in a narrow compafs, like the fmoke of a chimney in dry weather, almoft perpendicularly; but when the air is faturated with moifture, and becomes rarefied and expanded, as it always does before rain, the volume of air containing the effluvia will be extended horizontally, and diverge from thefe different bodies as from a centre, and will be fenfibly perceived on all fides, but will of courfe be moft perceptible on that to which the current of air or wind moves.

Vapors feen in winter over water.

In winter, when the thermometer is between 34 and 40 degrees, the air being in a ftate of condenfation, and the running

running water being warmer than the land, a mift or fog may be feen rifing above the rivers, particularly when the air is cold and clear; but this vapour is no longer vifible when the river is frozen, for though the ice be fubject to evaporation, it does not yield fo much vapour as water, and the water, in parting with its caloric in the moment of freezing, warms the furrounding air.

To the philofopher all objects in nature, both animate and inanimate, may afford both amufement and inftruction, particularly in meteorology; but to obferve them with due attention, we muft quit the bufy fcenes of life; "and thus our "lives exempt from public haunts, find tongues in trees, "books in rnnning brooks, fermons in ftones, and good in "every thing." Amufement and inftruction from the folitary contemplation of nature.

VIII.

Defcription of a Cheap, Simple, and Portable Inftrument, for determining the Pofitions of Objects in taking a Picture from the Life. By R. L. EDGEWORTH, *Efq.*

THAT active and intelligent philofopher and journalift, Cit. Pictet, author of the Bibliotheque Brittanique, on his late return from London to Paris, prefented various inftruments brought from this country to the National Inftitute of France. Among them was the inftrument here to be defcribed, and fince publifhed in the Bulletin des Sciences; on infpection of which my attention was excited to my notes, where, among other communications to be made to my readers, I find this inftrument, as publifhed in the excellent work "On Practical Education" by Maria Edgeworth and her father, to whom I have afcribed the contrivance in the title. The authors of the Bulletin affirm, that it was invented and executed by the children under the parental care of Mifs Edgeworth; but I find no fuch intimation in the original work. The inftrument of Profeffor Pictet is in various refpects inferior to that which I have here copied; infomuch that Cit. Cloquet has propofed an amendment, for giving the index an horizontal pofition when it is required to transfer the obfervation to paper, which is in fact lefs effectual than the provifions made for that purpofe,

Inftrument for perfpective prefented to the National Inftitute; was not as perfect as the original invention.

poſe and for fixing the true inſtrument. After this preface, I ſhall proceed to copy without farther remark, page 460.

Advantages of early habits of delineation, and the inſpection of drawings, &c. of machines.

"An early uſe of a rule and pencil, and eaſy acceſs to prints of machines, of architecture, and of implements of trades, are of obvious uſe in this part of education (mechanics). The machines publiſhed by the Society of Arts in London, the prints in Deſaguliers, Emerſon, le Spectacle de la Nature, Machines approuvées par l'Académie, Chambers's Dictionary, Berthoud ſur l'Horlogerie, Dictionaire des Arts et des Metiers, may, in ſucceſſion, be put into the hands of children. The moſt ſimple ſhould be firſt ſelected, and the pupils ſhould be accuſtomed to attend minutely to one print before another is given to them. A proper perſon ſhould carefully point out and explain to them the firſt prints that they examine; they may afterwards be left to themſelves.

Method of conveying the requiſite previous information,

"To underſtand prints of machines, a previous knowledge of what is meant by an elevation, a profile, a ſection, a perſpective view, and a (*vue d'oiſeau*) bird's eye view, is neceſſary. To obtain diſtinct ideas of ſections, a few models of common furniture, as cheſts of drawers, bellows, grates, &c. may be provided, and cut aſunder in different directions. Children eaſily comprehend this part of drawing, and its uſes, which may be pointed out in books of architecture; its application to the common buſineſs of life is ſo various and immediate, as to fix it for ever in the memory; beſides, the habit of abſtraction, which is acquired by drawing the ſections of complicated architecture or machinery, is highly advantageous to the mind. The parts which we wiſh to expreſs are concealed, and are ſuggeſted partly by the elevation or profile of the figure, and partly by the connection between the end propoſed in the conſtruction of the building, machine, &c. and the means which are adapted to effect it.

The art of perſpective requires a contemplation of the *mere picture* of external objects.

"A knowledge of perſpective is to be acquired by an operation of the mind, directly oppoſite to what is neceſſary in delineating the ſections of bodies; the mind muſt here be intent only upon the objects that are delineated upon the retina, exactly what we ſee; it muſt forget or ſuſpend the knowledge it has acquired from experience, and muſt ſee with the eye of childhood no farther than the ſurface. Every perſon who is accuſtomed to draw in perſpective, ſees external nature, when he pleaſes, merely as a picture: this habit contributes much

2 to

to form a tafte for the fine arts; it may, however, be carried to excefs. There are improvers who prefer the moft dreary ruin to an elegant and convenient manfion, and who prefer a blafted ftump to the glorious foliage of the oak.

"Perfpective is not, however, recommended merely as a means of improving the tafte, but as it is ufeful in facilitating the knowledge of mechanics. When once children are familiarly acquainted with perfpective, and with the reprefentations of machines by elevations, fections, &c. prints will fupply them with an extenfive variety of information; and when they fee real machines, their ftructure and ufe will be eafily comprehended. The noife, the feeming confufion, and the fize of feveral machines, make it difficult to comprehend, and combine their various parts, without much time, and repeated examination; the reduced fize of prints lays the whole at once before the eye, and tends to facilitate not only comprehenfion, but contrivance. Whoever can delineate progreffively as he invents, faves much labour, much time, and the hazard of confufion. Various contrivances have been employed to facilitate drawing in perfpective, as may be feen in "Cabinet de Servier, Memoirs of the French Academy, Philofophical Tranfactions, and lately in the Repertory of Arts." The following is fimple, cheap, and portable.

Young perfons who underftand perfpective may acquire much mechanical knowledge from drawings.

"Plate XVI. Fig. 1. A, B, C, reprefent three mahogany boards, two, four, and fix inches long, and of the fame breadth refpectively, fo as to double in the manner reprefented. Fig. 2, the part A is fcrewed, or clamped to a table of a convenient height, and a fheet of paper, one edge of which is put under the piece A, will be held faft to the table. The index P is to be fet (at pleafure) with its fharp point to any part of an object which the eye fees through E the eye-piece.

Defcription of the inftrument for fixing the points in perfpective views.

"The machine is now to be doubled as in Fig. 2, taking care that the index is not difturbed; the point, which was before perpendicular, will then approach the paper horizontally, and the place to which it points on the paper muft be marked with a pencil. The machine muft be again unfolded, and another point of the object is to be afcertained in the fame manner as before; the fpace between thefe points may be then connected with a line; frefh points fhould then be taken, marked with a pencil, and connected with a line; and fo on fucceffively till the whole object is delineated."

Its ufe.

The

Method of uſing a plain graduated ruler for taking views, &c.

The above machine affords a delineation which is ſtrictly accurate: but I take this opportunity of mentioning one ſtill more portable, though leſs exact, which may be uſed in taking ſmall ſketches in the field; where the table, and fixed ſheet of paper cannot always be ſuppoſed at hand. I do not know the contriver. It is merely a ſtrait flat ruler, having a diviſion of inches and ſmall parts (or any other diviſion) on its edge. A ſtring is faſtened to the middle of the ruler by paſſing it through a hole, and tying a knot on the other ſide; and at the other end of the ſtring there is a ſmall bead or knot to be held in the mouth. The length of the ſtring may be adjuſted at pleaſure; and when the ruler is uſed, it is held up at right angles to the ſtretched ſtring, ſo that its edge, as ſeen by one eye, may apply to any two objects; between which it will ſhew the diſtance to be afterwards transferred upon the paper by a ſcale, or by eſtimate.

Farther obſervations.

In this uſe of a graduated rule, it is moſt convenient and accurate to ſelect ſome one object in the picture for the point of ſight, and to meaſure all the diſtances from thence from the middle or beginning of the diviſions where the direction of the ſight is at right angles to the rule. And as this ſimple inſtrument does not give the inclinations, it may be beſt always to meaſure parallel or perpendicular to the horizon, and eſtimate the reſt. Indeed, the contrivance muſt be conſidered only as a ſubſtitute for the uſual method of eſtimating, and may be principally uſeful to aſſiſt in acquiring a correct judgment in this reſpect. W. N.

IX.

Concerning the new Planet CERES. *In a Letter from the Rev.* WILLIAM PEARSON, *including an Extract of a Letter from the* BARON DE ZACH *to Mr.* EDWARD TROUGHTON.

To Mr. NICHOLSON.

SIR,

Parſon's Green, March 9, 1802.

Introductory remarks. Popular account of the manner of applying obſervations.

AS the diſcovery of the new planet Ceres is an event which has already engaged the attention of the principal aſtronomers of Europe, and will continue to be a ſubject of diſcuſſion until

until the elements of its orbit ſhall be accurately aſſigned, it may be acceptable to many readers of your Journal to have a popular detailed account laid before them of the manner of applying obſervations to determine the whole period and form of the orbit, in order that they may comprehend, and conſequently feel an intereſt in, the peruſal of the different notices which will, moſt probably, be publiſhed from time to time in different countries.

How a planet is diſcovered—by its aſpect through the teleſcope;—or its motion.

When a heavenly body is firſt ſuſpected to be a planet, the ſuſpicion ariſes either from its apparent aſpect compared to a ſtar when viewed in a teleſcope, as was the caſe with Geo. Sidus, or otherwiſe on account of an obſerved change of relative ſituation, as was the caſe with the new planet Ceres when compared with ſome ſmall ſtars in its neighbourhood: the firſt thing to be done after the ſuppoſed diſcovery of a new planet is, to aſcertain its right aſcenſion and declination, and from thence its geocentric longitude and latitude; the means uſed for doing which need not be deſcribed here.

Subſequent obſervations. Theſe obſervations being made *from the earth* or geocentric, muſt be converted into heliocentric reſults, or ſuch as would appear *from the ſun.*

A ſeries of obſervations, and correſponding calculations, are uſually continued at ſucceſſive intervals, until the newly diſcovered body has advanced or receded through ſuch a portion of the ecliptic as to afford data for eſtimating its daily velocity when compared to that of the earth: now, if a ſeries of obſervations could be made by an obſerver in the ſun, the arc paſſed through by the planet would bear the ſame proportion to the interval of time elapſed between the firſt and laſt obſervations, that a circle does to the whole period, provided the motions were *equable*; and, if the motions were unequable, the obſerved progreſs at equal intervals would ſhew whether the inequalities were increments or decrements, and conſequently whether the planet was approaching the perihelion or aphelion point. But the obſerved places are *geocentric,* and muſt therefore be converted into heliocentric, to gain thoſe obſervations which an obſerver placed in the ſun would make;

Manner of doing this.

for which purpoſe the proportion to be uſed will be, by a ſimple caſe in plane trigonometry, *as the diſtance of the planet* (to be at firſt aſſumed) *from the ſun* : *is to the ſine of its obſerved elongation,* or angular diſtance from the ſun (or its ſupplement) : : *ſo is the earth's diſtance* from the ſun, taken from the tables, : *to the* ſine of an angle which is the *difference* between the heliocentric and geocentric places, and which is called the *parallax of the orb.*

The

From an heliocentric arc paffed over in a known time, its *approximate period* is deduced;—and thence by Kepler's law *a correcter diftance.* And the computations may thus be repeatedly amended.

The heliocentric arc, which the planet has paffed through in a given time, being thus *nearly* afcertained, the whole *approximate period* is next found on a fuppofition of equal arcs being paffed through in equal times; which will afford data for determining a fomewhat more accurate diftance of the planet from the fun, by the well known law of Kepler, by which the fquare of the periodic times are analogous to the cubes of the mean diftances of the earth and any other planet refpectively: an *approximate* diftance being thus obtained may, in the next place, be fubftituted for the *affumed* diftance, and the parallax of the orb be determined a fecond, and even a third time in this way; by means of which repetitions an approximate period, and a correfponding approximate diftance, will be obtained. Thefe will vary more confiderably from the truth the greater the diftance of the obferved arc is from one of thofe two points of the orbit where the planet has a mean motion; which points are always nearer to the aphelion than to the perihelion point, by a quantity which depends upon the eccentricity. If therefore it fhould fo happen that a new planet, at the time of its firft difcovery, were at, or very near, its mean diftance, the whole period and diftance obtained from a few of the firft obfervations would be pretty accurate.

Errors from excentricity,

corrected by determining the elementary points of the orbit, &c.

The next ftep to be taken is to determine the increments or decrements of motion, when a number of geocentric are changed into heliocentric places, and thus to trace the points in the ecliptic where the velocity is a *maximum*, where it is a *minimum*, and where it is a *mean*. Thefe will fhew where the perihelion and aphelion points are, and alfo the place of equated anomaly at an inftant when the equation is a maximum; thefe data afford the means of calculating the greateft equation and correfponding eccentricity; but before they can be affigned with accuracy, a confiderable time muft elapfe to afford the aftronomer an opportunity of obferving a few fucceffive oppofitions or conjunctions and ftationary points, for the purpofe of correcting the approximate elements, and of determining the true fhape and pofition of the orbit.

Determination of the obliquity and place of the nodes.

In the mean time the geocentric latitudes, gained by obfervation, muft be alfo converted into heliocentric latitudes, in order to determine the nodes or points where the two oppofite fides of the orbit crofs the ecliptic. For doing this the analogy

is

is—*As the ſine of the planet's elongation from the ſun : — is to the angle of commutation* (or difference between the helio. longitudes of the planet and of the earth)—*So is the tangent of the geocentric lat : is to the tangent of the heliocentric latitude.*

From this account of the means neceſſary to be uſed in aſcertaining the preciſe nature of the orbit of a newly diſcovered planet, it will be naturally inferred that the elements already aſſigned to the orbit of the new planet Ceres cannot be very accurate, allowing them to be duly proportioned to one another, becauſe an error in any one of its elements renders all the reſt erroneous; and there has not yet elapſed, probably, much more than one-fourth of an entire period ſince its diſcovery, nor has it yet been in the moſt eſſential portions of its orbit for affording the beſt data. Hence a continuation of accurate reports concerning this planet ought to be publicly recorded from time to time, in order that a compariſon of different and diſtant obſervations may afford the requiſite data for ultimately ſettling its elements with accuracy. I ſhall therefore make no apology for laying before your readers an extract from a letter of Baron Von Zach to his friend and correſpondent, Mr. Edward Troughton, mathematical inſtrument-maker, of Fleet Street, who has very obligingly put it into my hands, with permiſſion to make what uſe of it I may think proper. The letter is unuſually long, and full of intereſting matter; but I ſhall confine myſelf, in this communication at leaſt, to thoſe parts of it which principally relate to the new planet, and the ſubjects connected with it.

From the preceding account it follows that no elements of the new planet can yet be accurate.

Extract of a Letter from the Baron Von Zach to Mr. Edward Troughton.

Letter from Von Zach.

Gotha, January 28, 1802.

" ********** YOU have heard perhaps, dear friend, that I was ſo lucky as to diſcover again Mr. Piazzi's planet, called now, in honour of the king of Naples, *Ceres Ferdinandea.* I found this little planet firſt on the 7th of December laſt year, juſt between the head and the north wing of Virgo, in 178° 33½′ right aſcenſion, and 11° 41½′ declination N. An aſtronomical friend of mine, Dr. Olbers, in Bremen, found

Re-diſcovery of the planet Ceres.

found the planet on the 2nd of January between the N.^{o} 20 ♍ and ρ Virginis. I have not heard that this little planet, which resembles a star of the ninth magnitude in size, has been seen in France and England. I have already given information of my discovery to the president of the Royal Society, Sir Joseph Banks. I have the honour to send you here my observations of this planet.

Table of observations.

SEEBERG OBSERVATORY.

	Mean Time.			Ap. Right Ascen.			Declination.	
1801. 7 Dec.	18^h	48′	10,3″	178°	33′	30,6″	11°	$41\frac{1}{2}$′ N.
31	17	$38\frac{1}{2}$	: :	184	44	: :	11	5
1802. 11 Jan.	17	3	17,4	186	45	49,95	11	15
16	16	46	25,6	187	27	53,25	11	26
22	16	25	23,9	188	6	25,8	11	44
25	16	14	32,9	188	20	39,15	11	54
26	16	10	53,7	188	24	49,5	11	57

"If you, or your astronomical friends, wish to look at the new planet Ceres, which is rather difficult, this heavenly body being so small, I send you here a little ephemeris, which will direct you in the research of this planet. With my eight feet transit instrument, by the late Mr. Ramsden, and with 200 magnifying power, I could not perceive the least mark of a disk. Perhaps Mr. Herschel will see more: perhaps he will discover some satellites to Ceres.

Ephemeris.

Position of the Ceres Ferdinandea for Midnight, in the Seeberg Observatory.

	Right Asc.			Declin.		Rt. Af. in Time		
1802. Feb. 2	6^s	8°	43′	12°	31′	12	34	53
5	6	8	45	12	47	12	35	1
8	6	8	44	13	4	12	34	55
11	6	8	38	13	22	12	34	33
14	6	8	29	13	41	12	33	57
17	6	8	16	14	1	12	33	5
20	6	8	0	14	21	12	31	59
23	6	7	40	14	42	12	30	40
26	6	7	17	15	3	12	29	8
March 1	6	6	50	15	24	12	27	22

Difficulties in the observations.

"How much I want your four feet circle in my observatory, my present perplexity will show you. Hitherto I have observed all my zenith distances with a four feet Dollond's quadrant,

quadrant, and a circle of two feet from Cary; the telefcopes have little aperture, fo that I cannot fee the new planet Ceres, and confequently I cannot obferve the zenith diftances: as the air all January was very hazy and cold, I had much ado to fee the planet in my very excellent tranfit inftrument. I muft therefore wait till the planet fhall increafe in brightnefs, which yet will not go farther than to a ftar of the feventh magnitude. In the mean time I hope that by very clear weather I may diftinguifh the planet in my old quadrant, or in my little circle, and that this faint body will fupport the illuminating of wires, which has not been the cafe hitherto. This is the reafon that I could not obtain one exact declination, but my right afcenfions of Ceres are extremely exact. This difficulty will certainly never take place in my obfervatory when I once get your four feet circle. * * * * * * *

" As foon as the inftruments arrive, I fhall give you notice of it: in the mean while I have the honour to fubfcribe myfelf, with the greateft regard and higheft efteem, which the reigning Duke of Saxe Gotha very fincerely fhares with me,

Moft honoured Friend,

Your moft obedient Servant,

And devoted Friend,

FRANCIS BARON DE ZACH,

Lieutenant-Colonel, and Director of Seeberg Obfervatory.

" Before I finifhed this letter, I had two obfervations more of the new planet Ceres, viz. Lateft obfervations.

	Rt. Afc. Ceres.	Decl. Ceres.
28 Jan. 16^h 3′ 29″ M. T.	188° 31′ 37,85″	12° 9′ 41,3″
	tolerably well at the quadrant.	
29 15 59 43,7	188 34 18,13	12° 14′ only gueffed."

This extract affords me an opportunity of illuftrating the account, which has been juft given, of the application of two diftant obfervations of a planet for determining the heliocentric arc that has been run through in a given time: if I take the right afcenfion of Ceres for the 1ft of Jan. 1801, as given by Piazzi's

Application of the inftructions before detailed, to the obfervations on Ceres.

Piazzi's firſt obſervation, and alſo for the 26th of Jan. 1802, as given by the laſt obſervation of Von Zach in the preſent extract, and project the orbits of the earth and Ceres according to the eccentricity, mean diſtance and place of the aphelion of Gauſs, as given in the extract from the Moniteur in the different journals, we ſhall have the figure repreſented in Plate XIII. Fig. 1, which is thus conſtructed, viz.

Conſtruction of the orbits of the earth and Ceres, with the geocentric and heliocentric places of the latter, &c.

Suppoſing the point S to be the ſun, deſcribe, with any radius, the circle marked ♈, ♉, ♊, &c. for the ecliptic, and conceive it to be at an *infinite diſtance;* draw an occult line from S to ♋ $9\frac{1}{2}°$, the earth's aphelion, and another to ♒ $26\frac{1}{2}°$, the aphelion of Ceres; take 10 from a ſcale of equal parts, and deſcribe with that radius the innermoſt circle A ⊕ P from a point $\frac{1}{60}$ of the radius from S towards A in the occult line, and another circle with a radius of 27,6 of thoſe equal parts from the point C $\frac{1}{30}$ of the ſaid radius from S in the ſecond occult line, and theſe circles, which are eccentric with reſpect to the ſun at S, will very nearly repreſent the required orbits of the earth and Ceres, in both which A repreſents the aphelion, and P the perihelion points. In the next place find the two points in the earth's orbit, which are diametrically oppoſite the ſun's place for Jan. 1, 1801, at 9^h P. M. and Jan. 27, 1802, at 4^h A. M. which will be ♋ 11° 1′ 33″ and ♌ 6° 35′ reſpectively, and mark them as in the figure with their dates; after this mark the geoſcentric right aſcenſions of Ceres (denoted by the letter C with a croſs beneath) for Jan. 1, 1801, and G at ♉ 21° 47′ 48″, according to Piazzi, and for Jan. 26, 1802, at G at ♒ 8° 24′, according to Von Zach, and draw the two dotted lines from S to each; then if dotted lines be drawn parallel to theſe two lines from the earth (⊕) in Jan. 1801 and 1802 reſpectively, until they touch the orbit of the planet Ceres, theſe laſt lines will mark the geocentric *apparent places* of this planet in its *orbit,* of which the dotted lines from S to the ecliptic denote the *meaſure;* and, laſtly, if lines be drawn from S through the geocentric places to the ecliptic to H and H, they will indicate the heliocentric longitudes, and the arcs contained between the heliocentric and geocentric meaſures will be on each day the meaſure of what is called the parallax of the orb.—This method of converting geocentric into heliocentric places by projection is capable of great accuracy, and is, ſo far as I know, original.

The method of converting geocentric portion into heliocentric

The

The reason of drawing the occult *parallel* lines, which may not occur to the reader, is this; as the ecliptic is considered to be an infinite distance, so that the whole orbit of the earth, if seen from it, would appear only as a point, a line drawn to it from the sun, and another parallel thereto from the circumference of the earth's orbit, will, to an eye placed in the ecliptic, even if it were only at the distance of a star, appear *coincident* on the *same line*; hence if a line be drawn from the earth to the planet in its orbit, let their respective situations be what they may, another line drawn parallel to that from the sun, till it reaches the ecliptic, will shew the geocentric measure therein the same as if that measure were taken in a graduated ecliptic described from the earth as a center, and having all its divisions exactly parallel to those of the ecliptic described from the sun as a center.

by projection is considerably accurate;—and new.

Indeed, I have tried this projection with the geocentric and heliocentric places of some of the other planets taken from an ephemeris, and find it extremely accurate as well as easy.

Tried with the other other planets.

From this projection of the heliocentric and geocentric places of Ceres, it appears evident that the distance from the earth to it was much greater last summer when the earth was at P, than it is at present, and it is equally evident, that as the velocity of the earth is greater than that of the new planet, and as they were both moving in the same direction at the latter period in the projection, the distance will continue to diminish until both the planets are in the same strait line as seen from the sun, agreeably to what is said in the extract from Von Zach's letter; for the apparent magnitude is in an inverse ratio of the distance, so that as the distance diminishes, the apparent disk increases. The reason also appears clear, why there was very little apparent motion of the new planet in the last month as seen from the earth, for in this part of the earth's orbit Ceres would *appear* to have a retrograde motion, provided it were at *rest*, but its forward motion in a certain degree balanced that, and produced the effect of little or no apparent motion at all:—on the 6th or 7th of Feb. last it was stationary, and has been since retrograde in a small degree.

Remarks from the projection on the relative motions and apparent magnitude of the planet.

Determination of the heliocentric arc paſſed through,

In order to aſcertain by calculation the heliocentric arc paſſed through in 390,3 days, the time contained between Jan. 1, 1801, at 9 P. M. and Jan. 27, 1802, at 4 A. M. the heliocentric longitudes of the new planet may be aſcertained thus;

	S	°	′	″
1801, Jan. 1[d] 9[h] ſun's long.	9	11	1	33
Right aſcenſion of Ceres	1	21	47	48
Elongation	7	19	13	45

Then at 27 (ſuppoſed diſtance)	1,43136
Is to ſine of 49° 14′	9,87931
So is 10 (earth's diſtance)	1,00000
	10,87931
	1,43136
To ſine of paral. of orb. 16° 17′	9,44795

	S	°	′
Again 1802, Jan. 26[d] 16[h] ſun's long.	10	6	35
Right aſcenſion of Ceres	6	8	24
Elongation	3	28	11

Then as 27	1,43136
Sine of 61° 49′ (ſupplement)	9,94519
So is 10	1,00000
	10,94519
	1,43136
To ſine of paral. of orb. 19° 3′	9,51383

Geoſcentric place of Ceres.	S	°	′	″		°	′
Jan. 1, 1801	1	21	47	48	Parallax	16	17
Jan. 26, 1802	6	8	24	49	Ditto	19	3
Whole geocentric arc	4	16	37	1		35	20
Deduct ſum of parallaxes	1	5	20				
Whole heliocentric arc	3	11	17	1 in 390,3 days.			

and thence of the period 1387,27 days,

Now, if the motion of the planet were equable throughout its orbit, we ſhould have the whole tropical period from this ratio, as 101° 17′ 1″ : 390,3[d] : : 360° : 1387,27 days, but the portion

portion of a circle, which has been paſt ſince Jan. 1, 1801, is that in which the velocity has been above a mean velocity; therefore the period thus obtained is ſhorter than the true period by a quantity which depends upon the equation of the center, which has not yet been examined. At preſent want of leiſure, and the length of this paper, render it neceſſary that I ſhould defer entering upon the other particulars, which remain to be diſcuſſed, ſome of which, it is preſumed, will be found intereſting; for I propoſe to ſhow that ſome of the elements aſſigned by Gauſs, which are conſidered as the moſt accurate, are not in due proportion to one another. This diſcuſſion will therefore conſtitute the ſubject of another communication. In the mean time we may infer, either that the whole period is ſhorter than has been prematurely determined, or otherwiſe that the greateſt equation ſhould be much greater than the eccentricity at preſent aſſigned requires. Indeed, it will hereafter appear that the eccentricity, given from the Moniteur in the different journals, is almoſt *two-thirds* too little to correſpond to the equation which has been attributed to the new planet.

which, for reaſons given, is too ſhort,

though the period generally given muſt either be too long; or the greateſt equation muſt exceed what the aſſigned excentricity requires.

I am, SIR, as uſual,

Your's very reſpectfully,

W. PEARSON.

The early obſervations of Profeſſor Piazzi, to which Mr. Pearſon refers, are given by the profeſſor in the following tables. The computations in the latter table were made from elements which, at the preſent time, would require to be greatly amended; but I have not choſen to omit them. W. N.

Early obſervations of Piazzi.

Table

Table of the Mean Time, Right Aſcenſion, and Declination of the new Star as obſerved ; together with the Longitude of the Sun, and the Logarithm of its Diſtance from the Earth.

Right aſcenſions and declinations of Ceres Jan. and Feb. 1801.

	Days of the Month.	Ten Thouſandths of the day in mean Time.	Right Aſcenſion.			Declination.			Sun's Place.				Log. Diſtance of ⊕ from ☉
January.	1	3635	51°	47′	48·7″	15°	37′	43·5″N	9ˢ	11°	1′	33·1″	9,992617
	2	3606		43	27·7:		41	5·5		12	2	31·7	9,992629
	3	3577		39	36::		44	31·6		13	3	30·2	9,992641
	4	3547		35	47·2		47	57·6		14	4	29·0	9,992652
	10	3378		23	1·5:	16	10	32·0		20	10	29·5	9,992768
	11	3350		22	26·0		14	30, eſt.		21	11	29·5	9,992794
	13	3295		22	34·5		22	49·5		23	14	28·0	9,992848
	14	3268		22	55·8		27	5·7		24	14	27·3	9,992882
	17	———		27	35::		40	13·0		———	———	———	———
	18	———		28	45::		———	———		———	———	———	———
	19	3136		32	2·2		49	16·1		29	19	14·1	9,993060
	21	3084		38	34·0		58	35·9	10	1	21	2·5	9,993151
	22	3059		42	21·3	17	3	18·5		2	21	55·1	9,993196
	23	3033		46	43·5		8	5·5		3	22	46·4	9,993242
	24	———		51	45::		———	———		———	———	———	———
	28	2909	52	13	38·3		32	54·1		8	26	45·5	9,993522
	30	2860		27	2·1		43	11·0		10	28	10·6	9,993645
	31	2837		34	18·8		48	21·5		11	28	55·5	9,993708
February.	1	2813		41	48·0:		53	36·5		12	29	36·6	9,993773
	2	2789		49	45·9		58	57·5		13	30	17·0	9,993851
	4	———	53	7	45::		———	———		———	———	———	———
	5	2719		15	40·5	18	15	1·0		16	32	13·9	9,994083
	8	2650		44	37·5		31	23·2		19	35	2·2	9,994328
	11	2583	54	16	23·1		47	58·8 N		22	35	41·3	9,994588

N. B. The obſervations marked with two points (:) are a little doubtful; thoſe marked with (::) very uncertain.

Table

Table of the Geocentric Longitudes and Latitudes of the new Star, both by Obſervation and Calculation; together with their Differences.

Longitudes and latitudes of Ceres, Jan. Feb. 1801.

	Days of the Month.	Geocentric Longitude. Obſervat.	Geocentric Longitude. Calculat.	Differ.	Geocentric Latitude. Obſervat.	Geocentric Latitude. Calculat.	Differ.
January.	1	1s 23° 22′ 58·5″	1s 23° 21′ 59·2″	—59·3″	3° 6′ 32·4″	3° 6′ 50·2″	+17·8″
	2	19 44·8	18 40·2	—64·6	2 13·1	2 27·7	+16·6
	3	16 49·3	15 47·1	—62·2	2 57 58·9	2 58 63·3	+ 7·4
	4	14 16·5	13 18·9	—57·6	53 44·5	53 48·4	+ 3·9
	10	7 59·4	7 19·5	—39·9	28 50·9	28 31·8	—19·1
	11	8 25·7	7 43·4	—42·3			
	13	9 58·0	9 38·9	—19·1	16 49·0	16 21·0	—28·0
	14	12 1·6	11 32·9	—29·0	12 47·1	12 23·2	—23·9
	19	25 49·4	25 51·5	+ 2·1	1 53 28·3	53 1·3	—27·0
	21	34 21·8	34 23·4	+ 1·6	45 58·9	45 31·6	—27·3
	22	39 1·8	39 6·7	+ 4·9	42 18·7	41 51·3	—27·4
	23	44 15·6	44 17·4	+ 1·8	38 39·2	38 12·3	—26·9
	28	24 15 16·0	24 15 28·1	+12·1	20 58·7	20 32·0	—[illegible]6·7
	30	30 5·4	30 23·0	+17·6	14 5·3	13 43·5	—21·8
	31	38 8·6	38 20·5	+11·9	10 45·0	10 22·4	—22·6
February.	1	46 19·6	46 38·0	+18·4	7 23·8	7 3·6	—20·2
	2	54 55·6	55 12·8	+17·2	4 0·7	3 47·0	—13·7
	5	25 22 43·5	25 22 52·8	+ 9·3	0 54 19·0	0 54 9·6	— 9·3
	8	53 17·9	53 15·6	— 2·3	44 42·7	44 50·9	+ 8·2
	11	26 26 26·1	26 26 24·8	— 1·3	35 47·9	35 50·4	+ 2·5

X.

Entertaining chemical Experiments; with Notices of various new Facts and Obſervations reſpecting the Products of Nature and Art. By Mr. FREDERICK ACCUM. *Communicated by the Author.*

1. *Production of phoſphorated Hidrogen Gas* *.

As the uſual method of obtaining phoſphorated hidrogen gas, and exhibiting its ſpontaneous inflammation, requires a conſiderable ſhare of attention and manual dexterity, the amateur of chemiſtry will deem it not ſuperfluous to have no- Eaſy method of producing phoſphorated hidrogen.

* This and the five following Experiments were firſt noticed by Mr. Davy, who exhibited them in his lectures at the Royal Inſtitution.

ticed

ticed here the method of producing this gas in a more eafy, expeditious, and economical manner, by merely prefenting phofphorus to nafcent hidrogen.

Zinc, fulphuric acid, and phofphorus, afford the gas;

For this purpofe, let water be decompofed in the ufual manner, by means of zinc and fulphuric acid, and add to the mixture a quantity of phofphorus. The hidrogen evolved will diffolve part of the phofphorus; phofphorated hidrogen gas will be produced, and take fire at the furface of the fluid, fo long as the decompofition of the water is made with confiderable rapidity. But the gas produced in this procefs burns with a more lambent flame than that obtained in the ufual manner, probably on account of containing a larger quantity of hidrogen. The experiment is neverthelefs brilliant; for the gas is difengaged in fmall bubbles, which cover the whole furface of the fluid; they difengage themfelves rapidly, new ones are produced, and the whole fluid refembles a well of fire.

which takes fire fpontaneoufly, and continues to burn in a beautiful manner.

Particular inftructions.

For the fuccefs of this experiment, it is effential that the water, during the action of its decompofition, be confiderably heated, which may be effected by a copious addition of fulphuric acid, and that the phofphorus be prefent in a confiderable quantity. Half a part of phofphorus cut into fmall pieces, one of granulated zinc, three of concentrated fulphuric acid, and five of water, anfwer this purpofe exceedingly well.

2. *Phofphorated Hidrogen Gas burns with a green Light in nafcent hyperoxiginifed Muriatic Acid Gas, under the Surface of Water.*

Phofphuret of lime, hyperoxigenifed muriate of potafh, phofphorus, and fulphuric acid, produce *fire under water.*

Put into an ale-glafs, or Florence flafk, one part of phofphuret of lime, broken into pieces of the fize of a pea [not in fmall fragments or in powder], and add to it half a part of hyperoxigenifed muriate of potafh. Fill the veffel with water, and bring carefully into contact with the materials at the bottom of the fluid, three or four parts of concentrated fulphuric acid. This may moft conveniently be done, by letting the acid fall through a long-necked funnel, reaching to the bottom of the veffel, or by caufing it to pafs down the fides of it. As foon as the decompofition of the water, and that of the hyperoxigenifed muriate, takes place, flafhes of fire dart from the furface of the fluid, and the phofphuret illuminates the bottom of the veffel with a beautiful green light.

3. Combuſtion of phoſphorated Hidrogen Gas, by the Admixture of hyperoxigeniſed Muriatic Acid Gas.

If phoſphorated hidrogen and hyperoxigeniſed muriatic acid gas be mixed together, fire and flame are produced; and ſimple muriatic acid, water, and phoſphoric acid, are formed.

Flame by mixture of two gaſes.

4. Combuſtion of inflammable Subſtances in naſcent hyperoxigeniſed Muriatic Acid Gas.

Put into a wine-glaſs one part of hyperoxigeniſed muriate of potaſh perfectly dry; pour on it, two of colourleſs ſulphuric acid of commerce: a violent action will take place, and hyperoxigeniſed muriatic gas becomes evolved. If, during the extrication of this gas, one part of ſulphuric ether, alcohol, or oil of turpentine, be ſuffered to fall into the gas, an accenſion takes place, accompanied with a crackling noiſe.

Numerous combuſtions in hyperoxigeniſed muriatic gas:

In this manner not only all the inflammable fluid bodies, but likewiſe moſt of the ſolids, ſuch as camphor, reſin, tallow, pitch, elaſtic gum, &c. may eaſily be inflamed.

5. Combuſtion of expreſſed Oils at the Surface of Water, by Means of hyperoxigeniſed Muriatic Acid Gas.

Put into an ale-glaſs one part of hyperoxigeniſed muriate of potaſh; add to it three or four of water, and half a part of oil of olives, or of linſeed. On adding to it four or five parts of concentrated ſulphuric acid, a violent action takes place, much charcoal becomes depoſited, and a multitude of ignited ſparks paſs through the black fluid, exhibiting a beautiful phenomenon. On adding an additional quantity of hyperoxigeniſed muriate of potaſh and ſulphuric acid, the whole maſs takes fire, and burns with a denſe yellow flame.

of oils on water;

6. Combuſtion of Phoſphorus in hyperoxigeniſed Muriatic Acid Gas, under the Surface of Water.

Let fall into a wine-glaſs, or rather into a long cylinder, filled two thirds with water, one part of phoſphorus, and two of hyperoxigeniſed muriate of potaſh. On adding to this mixture three or four parts of ſulphuric or nitric acid, the phoſphorus takes fire, and burns vividly under the ſurface of the fluid: on agitating the mixture, ſtreams of ignited ſparks paſs through the fluid rapidly.

phoſphorus under water.

Remark,

Remark.—This and the two preceding experiments require caution. The operator ought to be diſtant during the affuſion of the acids, or the addition of the combuſtible body, which are ſometimes thrown out of the veſſel to a conſiderable diſtance.

7. *Decompoſition of ſulphureous Acid Gas, by Muriate of Tin.*

Decompoſition of ſulphuric gas by mur. of tin.

If ſulphureous acid gas, and freſh prepared muriate of tin, be brought into contact, the volume of the gas becomes ſpeedily diminiſhed, ſulphur is depoſited, and the ſimple muriate becomes converted into an oxigenated muriate of tin.

8. *Syrup of Violets, which has loſt its Colour, regains it, when agitated in Contact with Oxigen Gas.*

Syrup of violets reſtored by oxigen.

It is a fact well known to chemiſts, that ſyrup of violets is apt to loſe its colour by age, though not the leaſt perceptible change in the ſaccharine ſolution has taken place, when expoſed to light, or even when frequently agitated in contact with atmoſpheric air. In order to make the diſcoloured ſyrup regain its primitive blue colour, nothing more is neceſſary than to agitate it for a few minutes in contact with oxigen gas.

9. *Phoſphorated Hidrogen Gas is decompoſed by Light.*

Light decompoſes phoſphorated hidrogen.

Though phoſphorated hidrogen gas may be kept over mercury in the dark for any length of time unaltered, this is not the caſe if the gas be expoſed to light. In this ſituation the union of the phoſphorus and hidrogen is broken, the phoſphorus becomes ſeparated, and cryſtallizes in the veſſel, and the hidrogen is left behind.

10. *Acetite of Barites is capable of cryſtallizing.*

Cryſtals of acetite of barites.

A ſolution of acetite of barites, prepared by diſſolving pure barites in pure acetous acid, which had been expoſed to light in an open glaſs veſſel, was found to be converted into a ſolid regular cryſtalline maſs. The form of the cryſtals could not be determined, on account of their extreme minuteneſs.

11. *Spontaneous Reduction of Howard's fulminating Mercury.*

Spontaneous reduction of fulminating mercury.

Four ounces of Howard's fulminating mercury were placed ſtill wet on a chalk ſtone, expoſed to the rays of the ſun in a window. It was left in this ſituation unobſerved for at leaſt three

three months. The product during this time was converted into a black brilliant powder. On attempting to collect it together into one heap, and separating it from the paper which had been interposed, a globule of running mercury was seen. On introducing the powder into a bottle, and shaking it together, heat was evolved, and the whole mass became reduced to the metallic state.

12. *Production of red Sulphuret of Mercury in the humid Way.*

When equal quantities of a concentrated solution of oxigenated muriate of mercury, and concentrated fresh prepared fuming hidrosulphuret of ammonia, are mingled together, a brownish muddy precipitate is produced, which, when left undisturbed, turns yellow in three or four days, then orange, and at last acquires a beautiful cinnabar-red colour. The sulphuret of mercury, when separated, possesses all the properties of the best vermilion which is met with in commerce.

Cinnabar in the humid way.

13. *Air Bladder of the Carp contains common Air.*

The air bladder of the carp does not contain nitrogen, as Fourcroy, &c. assert. If the air of the bladder of this living fish be examined, it will be found to contain atmospheric air, and not nitrogen.

Error of Fourcroy concerning azote.

14. *Fish Bones contain much phosphoric Acid.*

The bones of fish contain upwards of one sixth part more of phosphoric acid, than those of quadrupeds. They may therefore advantageously be employed for making phosphorus.

Fish bones abound with phosphorus.

15. *Wax from Spiders.*

The yellow matter deposited in vessels containing spiders preserved in alcohol, is a true wax, which may be obtained from these animals by gently heating them.

Spiders' wax.

16. *Fluoric Acid dissolves Lead.—Method of obtaining it pure.*

Fluoric acid obtained in the usual manner, by decomposing fluate of lime by sulphuric acid in a leaden retort, always contains lead in solution; as may be proved by mingling the acid with a solution of water impregnated with sulphurated hidrogen gas. Tin vessels are less acted upon than those made of lead. The acid has no effect upon silver.

Common fluoric acid contains lead.

In

How to obtain it pure.

In order to obtain pure fluoric acid, one part of fluor ſpar reduced to a fine powder, may be mixed with two of ſulphuric acid, and one of water. The mixture is then to be introduced into a glaſs retort, to which a glaſs receiver has been previouſly luted, containing two ounces of water. Heat being then applied, the diſtillation is to be carried on ſlowly. After the decompoſition of the fluate of lime is effected, which may be known by the diſappearance of the whitiſh vapours in the retort, the contents of the receiver muſt be filtered, and diſtilled water added to the filtered fluid, till, on a new admixture of water, no further cloudineſs appears. The acid thus obtained contains no ſilex; it is abſolutely pure; and may be kept in glaſs bottles covered within with wax, or, in preference, with a hard varniſh.

17. *Benzoic Acid exiſts in Vanelloe.*

Cryſtalline matter on vanelloe.

A quantity of vanelloe pods [*Epidendrum Vanilla*, L.] which had been kept wrapt up into a bladder, and ſurrounded with thin ſheet lead for upwards of four years, were found to be covered with a white powder of a pungent ſaline taſte. On examining the internal parts of the wrinkled ſhell of the pods, the black ſeeds, when viewed under the magnifier, were found to be covered over with a multitude of oblong cryſtals, croſſing each other in all directions.

Experiments proving it to be benzoic acid.

In order to examine theſe cryſtals, one ounce of the pods were broken into ſmall pieces, and boiled in a Florence flaſk, with a quarter of an ounce of lime and ſix ounces of diſtilled water, for about ten minutes. The whole maſs was then ſuffered to repoſe, and the clear fluid, which was of an amber colour, and aromatic bitter taſte, was ſet aſide. The reſidue was treated in a ſimilar manner, and the fluid was added to the firſt. The clear ſolutions were then ſlowly evaporated to one fourth. During this proceſs a quantity of a brown adheſive ſubſtance ſeparated, which had all the properties of a true reſin; its weight amounted to nine grains. On letting fall into the concentrated ſolution, from which this reſin had been ſeparated, ſome pure muriatic acid, the whole became turbid, and a pulverulent yellow precipitate was depoſited, which increaſed in quantity on heating the mixture. All the precipitate obtained in this manner was then diſſolved in diſtilled water, filtered, evaporated, and cryſtallized; it yielded beautiful

tiful lemon-yellow cryſtals, which could not be obtained colourleſs, by repeated ſolutions and re-cryſtallizations. But after boiling them with charcoal powder, the fluid paſſed colourleſs through the filter, and yielded pure benzoic acid in the form of ſilky needles, weighing 23 grains.

18. *Camphor does not move upon Water at low Temperatures—Phoſphorus moves upon Mercury.*

If a baſon of water, upon which ſmall fragments oi camphor are in rapid motion, be ſuddenly transferred into a freezing mixture, the rotatory motion of the atoms of camphor inſtantly ceaſes. Camphor motionleſs on water in a very cold atmoſphere.

If a cylinder of dry phoſphorus be ſcraped with a knife over a ſaucer containing mercury free from duſt, &c. the ſmall particles of phoſphorus which are detached, and fall upon the quickſilver, ſpin in a ſimilar manner like thoſe of camphor placed upon clean water. Phoſphorus moves on mercury;

Camphor, benzoic acid, muſk, caſtor, civet, aſſafœtida, vanilloe, and various other odoriferous ſubſtances, remain motionleſs when placed upon this fluid. but camphor, &c. do not.

19. *Frogs change their Colour, and decreaſe in Size, when ſecluded from Light, and die when ſuddenly expoſed to vivid Light.*

A number of male frogs of the largeſt ſize, and of a beautiful yellowiſh brown colour, which had been kept for upwards of ſix months in a reſervoir, ſituated in a dark place, loſt their colour during that time, became perfectly black, and had decreaſed in ſize to leſs than one half. Frogs become black and ſmaller by keeping in the dark. A number of theſe animals, which were very briſk, and apparently in a good ſtate of health, were expoſed to the rays of the ſun, in order to ſee if they would regain their colour. Light kills them They all died, during the experiment, in about three hours. The galvanic influence could not ſtimulate their dead muſcles into action. and deſtroys all irritability.

A frog, which had eſcaped, was found in a veſſel containing a concentrated ſolution of potaſh, freed from carbonic acid, in which, from the time of diſcovery, he could not have remained longer than three hours. A frog converted by potaſh into a ſoluble clear maſs. The animal was much diſtorted, and converted into a fleſh-coloured gelatinous maſs. It was beautifully tranſparent, and twice its natural ſize. On pouring boiling water upon it, the whole was diſſolved into a jelly.

Habitudes of these animals in different fluids and gafes.

Frogs kept in diftilled water foon become languid, and die. They cannot live in water impregnated with nitrous oxide, nor in water holding carbonic acid gas in folution in confiderable quantity. They live in an atmofphere of pure nitrogen for five or fix days, if water be prefent. They inftantly die in all hidrocarbonates; but may be kept alive in nitrous gas for fome days.

XI.

The Method of cryftallizing Lime. By TROMMSDORFF *.

Hiftory of the cryftallization of lime.

THE cryftallization of lime was firft difcovered accidentally by Schaub. Bucholz alfo obtained very fine cryftals of this earth by boiling it with its muriate. I have verified this difcovery; and find that lime may be obtained in cryftals equally well in winter as in fummer; with the exception, that the falt prepared during the winter feafon forms cryftals which are thicker and larger. In order to obtain thefe cryftals, any quantity, at pleafure, of the muriate of lime is to be boiled with one fourth, or even lefs, of cauftic lime, and the fluid concentrated until in winter a drop of the folution, let fall upon a cold ftone, fhall acquire the confiftence of fyrup without cryftallizing or congealing. The fluid is then to be ftrained through a clofe cloth filter into a capfule of porcelain or earth, which is then to be covered with a fimilar capfule, or a wooden cover, in order that the cooling may be as flow as poffible. By this means very long but flender cryftals of cauftic lime are obtained, which muft be wafhed in alcohol to clear off the adherent muriate. This operation muft not be attempted with a lefs quantity than feven pounds of muriate of lime.

How effected.

Refidue, after diftilling ammonia from much lime, is the cryftallized earth.

It is well known, that when muriate of ammonia is diftilled with excefs of lime, for its decompofition, part of the refidue adheres fo ftrongly to the retort, that it is almoft impoffible to detach it by foftening it. This hard mafs is for the moft part formed of lime confufedly cryftallized, which is more difficultly diffufed in water than the fame earth in powder, or a ftate of minute divifion.

* Journal der Pharmacie, vol. ix. part 1. p. 108.

XII.—*On*

XII.

On the Cryſtallization of the Hidroſulphuret of Soda. By Cit. Vauquelin *.

Berthollet's obſervations, that ſulphurated hidrogen performs the functions of an acid.

BERTHOLLET, in a memoir which he communicated to the Inſtitute about four years ago, ſhewed that ſulphurated hidrogen has ſeveral properties in common with the acids, ſuch as that of reddening the tincture of turnſole, uniting with the earths, the alkalis, and the metallic oxides, and forming cryſtallizable combinations with ſome of theſe ſubſtances.

Sulphurated hidrogen forms with ſoda a cryſtallizable ſalt;

I have had occaſion to obſerve, a few days ago, one of the combinations of this kind, namely, that of ſulphurated hidrogen with ſoda. Having lixiviated a conſiderable quantity of ſoda, manufactured by Citizens Payen and Bourlier, to extract the carbonate of ſoda, I left the concentrated mother water in a corner of the laboratory. At the end of ſome decads, I found at the bottom of this liquor a white tranſparent ſalt, cryſtallized in rectangular tetahedral priſms, terminated by four-ſided pyramids, ſome of which were octahedral. As this is not the form aſſumed by carbonate of ſoda, I made ſome experiments to determine its nature. Its taſte was at firſt acrid and cauſtic, nearly reſembling that of the alkalis, which led me to ſuſpect that it might be cauſtic ſoda; but I was ſoon undeceived, by an inſupportably bitter taſte which ſucceeded, and by a ſlight odour of ſulphurated hidrogen gas.

of an acrid, cauſtic taſte; ſucceeded by extreme bitterneſs;

It is abundantly ſoluble in water; and notwithſtanding the cauſticity, which ſeems to announce that the alkali is united with a ſlight acid, nevertheleſs it abſorbed caloric in its ſolution. Its ſolution had no colour, but the ſmell of ſulphur was ſtronger than that of the ſalt itſelf; the acids produced a lively effervefcence, and developed a very ſtrong ſmell of ſulphurated hidrogen gas. But the fluid did not become turbid: the nitric and oxiginated muriatic acids, on the contrary, formed an abundant precipitate, which, when waſhed and dried, exhibited all the characters of ſlightly hidrogenated ſulphur. The ſalt, or its ſolution, ſpread on blotting paper, ſoon aſſumed a dark green colour. Laſtly, this ſalt precipitates all the metallic oxides from their ſolutions, perfectly ſi-

very ſoluble in water, and producing heat; effervefcing with acids; depoſiting ſulphur by the nitric and ox. mur. acids; dark green as it dries; precipitates metals like an hydroſulphuret;

* Annales de Chimie, No. 122, vol. xli. p. 190.

milar to thoſe which the ſame ſubſtances aſſume when precipitated by the artificial hidroſulphuret of ſoda. It does not precipitate the earths when diſſolved in the acids, excepting alumine, zircone, and yttria; which proves that the alkali is perfectly ſaturated with ſulphurated hidrogen gas.

but does not throw down the earths; whence the alkali is ſaturated.

This ſalt, when decompoſed by ſulphate of iron, afforded hidroſulphuret of iron, and ſulphate of ſoda, which were obtained cryſtallized by evaporation and cooling of the liquor; whence this ſalt is a true hidroſulphuret of ſoda.

With ſulphate of iron it affords hidroſulphuret of iron, and ſulphate of ſoda.

The preſence of this ſalt in the ſoda of Meſſrs. Payen and Bourlier, proves that theſe manufacturers did not uſe a ſufficiently large quantity of chalk to ſaturate the whole of the ſulphur ariſing from the decompoſition of the ſulphate of ſoda by charcoal; unleſs we ſuppoſe that it is formed during the decoction the alkali is made to undergo when extracted from the carbonate of ſoda.

The manufacturers did not uſe chalk enough, &c.

The whole novelty of this obſervation conſiſts in the property which this ſalt has of cryſtallizing, and the determination of its figure. Its other properties have been deſcribed by Citizen Berthollet with great exactneſs.

Remark.

XIII.

Memoir reſpecting a new Combination diſcovered in Zaffre, which Cit. Brugnatelli *ſuppoſed to be a peculiar Acid, which he denominated the Cobaltic Acid. By* Cit. Darracq *.

CITIZEN Brugnatelli, in a memoir printed in the Annales de Chimie for the month of Pluvioſe in the year 8, page 113 †, relates various experiments upon zaffre, or the grey oxide of cobalt, from which he concluded that this ſubſtance contains a new acid. I ſhall deſcribe ſome of theſe experiments, as well as certain properties attributed to his cobaltic acid.

Suppoſed diſcovery of a new acid in cobalt, by Brugnatelli.

He took zaffre, which he left in digeſtion with ammonia. After ſome days maceration in the ſun he obtained a fluid of a red colour, known by the name of ammoniuret of cobalt, which he filtered and evaporated to dryneſs. The concrete reſidue thus obtained appeared to him to conſiſt of two very diſtinct ſubſtances, one of a deep red colour, and the other of

He digeſted zaffre in ammonia; filtered and evaporated to dryneſs. The reſidue gave oxide and the ſuppoſed new acid, united with ammonia.

* Annales de Chimie, No. 121. Vol. XLI. p. 66.

† Philoſ. Journal, quarto, IV. 36.

a pale

a pale yellowifh tinge. The red part was foluble in water, and the yellowifh remained upon the filter. This refidue he confidered as the pure oxide of cobalt. The foluble part was evaporated, and by cooling it depofited certain fmall cryftals, which Citizen Brugnatelli inferred to be a combination of the new cobaltic acid with ammonia. The fupernatant liquor alfo poffeffed evident characters of acidity.

It is either coloured or colourlefs.

The author obferved, that his acid might be obtained either coloured or colourlefs, according to the methods ufed in procuring it. Evaporations made by fire left a refidue foluble in water, and afforded a cobaltic acid nearly colourlefs, whereas thofe made in the light of the fun always afforded it more or lefs red. Citizen Brugnatelli being defirous of afcertaining whether his acid was formed during the operation here defcribed, or whether it exifted ready formed in zaffre, boiled for twenty-four hours fix pounds of this fubftance in eight pounds of water. He filtered the liquid while hot and evaporated it. When it was reduced to half it became turbid; the evaporation was continued until no more than one third of the fluid remained. It was then taken from the fire and depofited a white fubftance, which was collected upon the filter. The fluid which paffed was of a bright yellow colour, with a fenfibly acid tafte; it acted in all refpects the fame as the cobaltic acid obtained by the procefs before pointed out.

Zaffre boiled with water afforded it.

Properties ftated as characteriftic of this acid.

The following are among the properties which Citizen Brugnatelli confiders as characteriftic.

1. It precipitates the folution of filver.

2. It precipitates lime water in a white coagulum infoluble in water, and in an excefs of acid.

3. It is feparated from its aqueous folution by alcohol.

4. It precipitates the acetite and muriate of barites.

New experiments.

I fhall now proceed to defcribe the experiments I have made on this fubject, and prefent to the Inftitute the confequences which I deduce from the fame.

Experiment I. The ammoniuret was formed.

Experiment 1. I formed the ammoniuret of cobalt by leaving zaffre in digeftion in the fun's light in ammonia. I was careful to agitate the mixture which was included in a mattrafs, and foon obtained a red colour, that after forty-eight hours became a bright red. I obferved in this fluid a cryftallization in confiderable abundance, in the form of white brilliant needles. This cryftallization was permanent till the fun had

had again heated the fluid, when it became diſſolved in the liquid. In this ſtate the ammoniuret of cobalt was filtered, and put into a retort; in proportion as its temperature increaſed it aſſumed a violet red colour which grew deeper and deeper, and at length had the appearance of the bright colour of red wine. When the greateſt part of the ammonia was evaporated the mixture acquired a greeniſh colour, and by repoſe a ſubſtance of the ſame colour as the ſolution was precipitated. The liquid filtered while hot and evaporated to dryneſs, afforded rudiments of cryſtals, of which the form could not be determined. White and brilliant parts were obſerved, and the reſt of the matter was of a yellowiſh colour. Upon this reſidue I poured water, and agitated it with a ſpatula of platina. The leaſt coloured portion was totally diſſolved, and communicated a ſtraw colour to the water. This fluid was acid, and poſſeſſed ſome of the properties announced by Cit. Brugnatelli. I ſhall preſently deſcribe the experiments to which I ſubjected it, the reſults of which properly examined, prove that it is not an acid formed by the cobalt. The two reſidues obtained in this experiment, one of a greeniſh, and the other of a yellowiſh colour, were proved to be not pure oxide of cobalt, as Citizen Brugnatelli affirmed, but a combination of that oxide with the arſenic acid.

The ammonia was evaporated by heat.

A precipitate fell and the filtered fluid afforded cryſtals by evaporation;

Part of the reſidue was taken up by water.

It was acid,

but it was not cobaltic acid.

Experiment 2. Having prepared a new quantity of ammoniuret of cobalt as in the former experiment, I ſubmitted to ſpontaneous evaporation in a ſituation where the ſun accelerated the volatilization of the ammonia. In proportion as the liquor evaporated a flocculent matter of a whitiſh roſe colour fell down, which ſenſibly increaſed until the ammonia was evaporated. The fluid emitted no ſmell, but it had preſerved a roſe colour of a conſiderable beauty. I filtered it to ſeparate the precipitate, and again evaporated the liquor to dryneſs. The remaining matter was diſſolved in diſtilled water. This ſolution of a light roſe colour was acid, and poſſeſſed the properties of the acid obtained in the former experiment,

Exp. II. The ammoniuret of cobalt was expoſed to the ſun to loſe its ammonia.

Flocculent precipitate.

The clear fluid filtered and evaporated to dryneſs left a matter ſoluble in water, which was acid, and the ſame as in Exp. I.

The reſidue upon the filter was carefully examined, and aſcertained to be the arſeniate of cobalt.

The firſt reſidue on the filtre was arſeniate of cobalt.

Experiment 3. I took, as the author of the memoir himſelf did, one kilogram of zaffre, which was pulveriſed till it became an impalpable powder; I boiled it with three litres of diſtilled water for half an hour, and filtered the liquid while hot.

Exp. III. Powdered zaffre was

hot. The filtered fluid had a light colour, and perceptible taſte. I evaporated it in a capſule of porcelain, and towards the end of the evaporation it became turbid. I continued to evaporate until the fluid was reduced to about one hectogram by cooling, and obtained needle formed cryſtals. The mixture was filtered with agitation; the cryſtals remained upon the filter, and the fluid paſſed very tranſparent of a bright yellow colour. Citizen Brugnatelli in making this experiment has not remarked the cryſtallization; he has only obſerved a white matter which was converted to a roſe colour by the contact of the air, and ſuppoſed by him to be the oxide of cobalt. The needle formed cryſtallization which I obtained afforded a conſiderable diſengagement of arſenic as ſoon as it was heated. When carefully examined it was proved to be the arſeniate of cobalt, and not the oxide as had been announced. This fact is no doubt of little conſequence to the diſcoveries of Citizen Brugnatelli; but nevertheleſs it might ſerve to ſupport my opinion, if the reſults of my experiments were not more than ſufficient to prove that the cobaltic acid has no exiſtence.

boiled in water and filtered.

Cryſtals obtained by evaporation.

It gave out arſenic by heat; and was arſeniate of cobalt.

The liquids of the 1ſt, 2d, and 3d experiments, which proved to be acid, and of the ſame nature, were ſubjected to the following trials, and comparatively with the arſenic acid.

Examination of the acid liquors.

Firſt trial. This acid liquor is precipitated by ſulphurated hidrogen, and by the alkaline hidro-ſulphurets of a yellow colour ſimilar to opiment, or the ſulphuret of arſenic. This precipitate is in fact ſulphuret of arſenic, and not ſulphur precipitated, as Citizen Brugnatelli has ſuppoſed.

Sulphurated hidrogen throws down ſulphuret of arſenic.

Second trial. It precipitates the ammoniuret of copper of a blueiſh green colour. This property belongs to the arſenic acid. The combination is known by the name of arſeniate of copper.

It forms arſeniate of copper with the ammoniuret of that metal.

3. Sulphate of copper mixed with this fluid affords a precipitate of the ſame colour as the ammoniuret of this metal. The arſenous acid alſo poſſeſſes this property not ſo eminently as the pretended cobaltic acid, but a ſimilar precipitate may be obtained by uſing the arſeniate of cobalt formed artificially. In this caſe the reſults are the ſame, and no difference can be perceived.

Its effect on ſulphate of copper is the ſame as that of the arſeniate of cobalt.

4. It precipitates the nitrate of ſilver of a white colour. The arſenic acid alſo enjoys this property.

It alſo acts like arſeniate of cobalt on nitrate of ſilver;

and of mercury; 5. The nitrate of mercury is thrown down of a ſtraw colour; and this phenomenon is alſo preſented by the arſenic acid.

and lime water; 6. It precipitates lime water in a white coagulum inſoluble in water, but not inſoluble in an exceſs of acid, as Citizen Brugnatelli affirms; for in fact, it is re-diſſolved with the ſame facility in the pretended cobaltic acid as in arſenic acid. I think the miſtake of Citizen Brugnatelli ariſes from his not having uſed a ſufficient quantity of acid.

and acetite and muriate of barites; from impurity. 7. It precipitates the acetite and the muriate of barites. I am aſſured that this precipitation aroſe from a ſmall portion of ſulphuric acid contained in the acid called cobaltic. The arſeniate of cobalt alſo renders theſe ſolutions cloudy.

Precipitates infuſion of galls. 8. It forms an abundant yellowiſh precipitate with the tincture of nut gall newly made. It is known that the arſenic acid produces the ſame phenomenon.

The pretended cobaltic acid is ſeparable from water by alcohol, and even by alcoholic ſolution of arſenic acid. 9. The laſt trial. This laſt experiment appears to have been conſidered by Citizen Brugnatelli as the moſt characteriſtic of his cobaltic acid. He found that alcohol ſeparated his acid from its aqueous ſolution, and alſo that the arſenic acid diſſolved in alcohol enjoys the ſame property; by theſe proceſſes he obtained his concrete acid. This property of the cobaltic acid at firſt ſurpriſed me; but preſuming that I ſhould diſcover the explanation by examining the acid thus precipitated, I ſubjected it to the following experiments.

This precipitated acid gave out arſenic by heat; 1. When heated upon charcoal by the blow pipe it emitted white vapours very eaſily diſtinguiſhable to be thoſe of arſenic.

rendered borax violet; 2. A ſmall portion heated with borax communicated a violet colour.

The precipitating alcohol has acquired arſenic acid. 3. This acid being precipitated by alcohol alone I examined the liquid, after having ſeparated it by the filter. Sulphurated hidrogen produced an abundant yellow precipitate, which was found to be a ſulphuret of arſenic.

The acid precipitate was nearinſoluble in water; 4. The ſeparated acid was ſcarcely at all ſoluble in water.

but became ſoluble by addition of arſenic acid. 5. When mixed with a few drops of arſenic acid it diſſolved completely, and reſumed its original properties.

Hence the acid of Brugnatelli was arſeniate of cobalt diſſolved From theſe trials I concluded, that the acid of Citizen Brugnatelli could be nothing elſe but the arſeniate of cobalt diſſolved in an exceſs of acid.

To put this beyond doubt I diſſolved pure oxide of cobalt in the arſenic acid; I evaporated the ſolution to dryneſs, and added

added diftilled water to the refidue. After agitating the mixture it was filtered. The liquid thus obtained had a flight rofe colour, and poffeffed all the properties of the cobaltic acid.

in excefs of acid.

A direct combination of oxide of cobalt and arfenic acid, had the colour, the precipitability, and all the properties of the fuppofed cobaltic acid.

This acid when mixed with alcohol afforded an abundant precipitate, and the precipitate when collected prefented all the phenomena exhibited by the concrete cobaltic acid.

Inference.

It appears therefore certain from the refults I have here detailed, that the fo called cobaltic acid has no exiftence; that the fimple combination of arfenic acid with oxide of cobalt has led Citizen Brugnatelli into an error, becaufe this combination with excefs of acid, is the fubftance obtained from zaffre by the methods defcribed in this memoir.

XIV.

On the Plumb Line and Spirit Level, by EZEKIEL WALKER. *Communicated by the Author.*

Lynn Regis, March 18, 1802.

Importance of the adjuftment of inftruments.

WHEN it is confidered that the accuracy of aftronomical obfervations depends greatly upon the exact manner of adjufting the inftruments, any improvement in the method of making thofe adjuftments may not be thought unworthy of a place in a philofophical Journal.

Plumb-line and fpirit-level.

The plumb-line and fpirit-level are much ufed by aftronomers, but from the great improvements made in the conftruction of the latter, the plumb-line feems to lofe that preference which it had fome years ago.

Common fpirit level faid not to exceed the precifion of one minute.

Mr. Ludlam fays *, " I believe the fpirit-level cannot afcertain the pofition of the axis" (of an inftrument) " nearer than to a minute if fo near." And when I firft began to ufe the fpirit-level for determining the pofition of the axis of a tranfit telefcope, I found it nearly in the fame imperfect ftate as mentioned by Mr. Ludlam: I was therefore very defirous of having fome better method of making this adjuftment.

Plumb-line to a tranfit inftrument.

After having thought much upon the fubject, I fixed on having a plumb-line fufpended from the object end of the telefcope,

* Aftronomical Obfervations, page 42.

telefcope, to pafs over a point near the eye end. And in this manner a plumb-line was applied to it by Mr. E. Troughton, with his ufual accuracy.

Defcribed by Dr. Ufher.

This way of levelling the axis of a tranfit telefcope has fince been defcribed by Dr. Ufher in the Tranfactions of the Royal Irifh Academy.

Laid afide.

The Dr. fpeaks unfavourably of it, but many of his objections are ill founded, and it is probable that he never tried it. This method of applying the plumb-line gave me much fatisfaction at that time, but fome years afterwards it was totally laid afide for a ground fpirit-level. The fpirit-level which I now ufe is ground to a radius of 430 feet, with two indexes made of ivory, each about one-third of a minute in length, graduated to every two feconds. Thefe are made to flide upon the level, that they may be brought over the ends of the bubble at pleafure; and no other adjuftment is neceffary.

Spirit level of 430 feet radius, with indexes graduated to two feconds.

Improved method of ufing it.

My method of ufing the fpirit-level is not only more convenient than any other that I have met with, but the axis of an inftrument may alfo be examined by it without altering the perpendicular fcrew, which is a matter of fome confequence; becaufe an inftrument newly adjufted is fubject to vary for fome time after, by the changes which take place in the temperature of the air; confequently the adjufting fcrews fhould never be touched, except to make fome neceffary alteration.

Obferve one end of the bubble; reverfe the level, and obferve again. The half difference of ftation is the error; to which bring the inftrument.

My method is as follows. Having hung on the level, flide an index over one end of the bubble, and after it has remained at reft about a minute read off from the index the number of feconds *a* pointed out by the end of the bubble, then take off the level, invert the ends, and hang it on again, and if the fame end of the bubble come to the fame divifion (*a*) as before, the axis is level. But if the end of the bubble reft at any other divifion (*b*) on the index, then half the difference will be the error; and by means of the perpendicular fcrew, bring the end of the bubble half way between *a* and *b*, and the axis will be level without further trouble.

If the pofition be alfo determined by the other end of the bubble, the difference is feldom half a fecond.

The pofition of the axis may be alfo determined at the fame time, by the other end of the bubble, and if the level be a good one, the refults given by the two ends will feldom differ fo much as half a fecond.

In

In confequence of an accident which happened to my fpirit-level, I was under the neceffity of having recourfe once more to my plumb-line.

Plumb line refumed.

But Mr. Nicholfon's paper on page 134, Vol. I. of his Journal, (quarto) made me look upon it with a very fufpicious eye. My having been affured that my wire was of the beft quality, did not remove thofe fufpicions which that paper had raifed in my mind of its being crooked. At laft a method of trying this by experiment occurred to me, which was by reverfing the fides of the wire, thus: after having made the wire to bifect the point near the eye end of the telefcope, I turned that fide of the wire which faced the fouth, to face the north, which was done very eafily, by reverfing that piece of brafs to which the wire is fixed at the top, and at the fame time turning the plumb line half round the fame way. To my great furprize I found that my wire did not form an accurate right line by feven feconds, though it was loaded with a proper weight.

Sufpicion of flexure in the wire.

How verified.

The error from flexure was feven feconds.

I tried another piece of wire from the fame bobbin, which deviated fix feconds from a right line; I tried feveral other pieces before I found one that ftood the teft of this examination.

But after the error of a plumb-line has been thus determined, it is as good as if it were perfectly ftraight.

Remedy. But qu. if the error be invariable? W. N.

Another inconvenience that attended my plumb-line was this,—though the wire hung as near the point as poffible without touching, yet the microfcope could not be adjufted to view them both diftinctly at the fame time. An accident, however, put it in my power to remove this difficulty. Levelling the axis of the tranfit, one day, without any light in the room, except that which came through the opening in the roof behind me, I obferved the fhadow of the wire upon the telefcope. By placing the microfcope a little on one fide of the wire, I faw the fhadow very black and perfectly well defined. The fhadow and the point being on the fame plane, both objects were diftinctly feen with the fame adjuftment of the microfcope.

Difficulty of obferving the wire and the ftroke at the fame time.

Removed by ufing the fhadow of the wire.

The length of this plumb-line from the notch in which the wire refts at the top to the *point* is about 41½ inches. The wire fubtends an angle of 24 feconds at the point, and the point itfelf 32 feconds, confequently four feconds of the point are feen on each fide of the fhadow when the wire bifects the point.

Eftimate error of adjuftment by this plumb line.

point. Though thefe eight feconds meafure only .00159 parts of an inch, yet an experienced obferver will eftimate with confiderable exactnefs when the parts of the point feen on each fide of the fhadow are equal. Still it is not improbable, but a fecond or two may fometimes efcape his notice on fo fmall a fcale.

Spirit level much more accurate.

The fpirit-level has the advantage of fo large a fcale, that a fecond may be divided by the eye into many equal parts, and it is fo eafy in its application, that a few minutes only are fufficient for determining the pofition of the axis of an aftronomical inftrument, to lefs than a fingle fecond, as appears by the following obfervations.

Obfervations of the level,

Pofition of the Axis fhewn by the Spirit-Level.

By the end x.	By the end y.	
$''$	$''$	$''$
1.75 -	- 1.7	$x + y \div 2 = 1.72$
1.65 -	- 1.85	$x + y \div 2 = 1.75$
2.1 -	- 2.2	$x + y \div 2 = 2.15$
1.7 -	- 1.77	$x + y \div 2 = 1.73$
1.6 -	- 1.6	$x + y \div 2 = 1.6$

Eaft end of the axis dips - - - - 1.79

were fairly taken and reported.

Thefe ten obfervations are independent of each other, they were all taken at the fame time, and none were rejected to make a fhow of precifion, inftead of fhewing the truth.

Other levels of Troughton equally accurate.

I have had other fpirit-levels ground by the fame ingenious artift *, which were as good as this, though one of them was filled with fpirits of wine.

* Mr. Edward Troughton, Fleet-Street, London. He in general ufes ether.

XV. *Defcription*

XV.

Defcription of a Machine for raifing Ore from Mines. By Mr. T. ARKWRIGHT *, *of Kendal, Weftmoreland.*

A, Plate XV. is an endlefs chain formed of thin plates of iron, through the ends of which plates iron bolts are paffed, which keep the fides of the chain at a certain diftance afunder, and on which the buckets to contain the ore are fufpended. B C D E, the buckets fufpended on the iron bolts, G H I, three cylinders, round which the chain and buckets revolve. The two cylinders G H are placed above the fhaft; the cylinder I within the mine. Their rims are fo much higher than the body of the cylinders, as to admit the buckets to lie within the rims.

Conftruction of a machine for raifing ore from mines.

As the endlefs chain and buckets are moved forwards by a power applied to the axis of the cylinder G, the bolts of the chain fall into notches made at regular diftances in the rims of that cylinder, which preferve the chain from flipping.

As each empty bucket paffes under the axis of the bottom cylinder I, it loads itfelf with ore inftantaneoufly from a large box K, conftantly filling by the workmen below, which box refts on two moveable pins L, at that end furtheft from the wheel, and on an iron catch M at the other. The bucket thus filled afcends to the top of the cylinder G: and, in its paffage betwixt the cylinders G and H, difcharges its contents into a channel or receiver placed betwixt them, from whence they flide into a cart or receptacle placed underneath the inclined trough N. The empty bucket paffes over the cylinder H, defcends on the oppofite fide under the cylinder I, and loads itfelf again at K, as before mentioned; the buckets regularly loading and difcharging themfelves, whilft the cylinder G is kept in motion.

O is a racket-wheel on the cylinder, to prevent a retrograde motion in the chain.

* From "Tranfactions of the Society of Arts" for 1791. p. 277. The Society gave Mr. Arkwright a premium of twenty-five guineas for this invention.

Fig. 2.

Fig. 2. ſhews, upon a larger ſcale, the manner in which the box K above mentioned loads the buckets. P is an iron tooth projecting from the endleſs chain, which, preſſing upon the catch R, underneath the box K, occaſions that part of the box next the chain to ſink down, and diſcharge into the bucket beneath it a quantity of ore ſufficient to fill it. As the loaded bucket riſes, it lifts the box K to its former place, till the operation is repeated by the next tooth upon the chain.

XVI.

Curious Properties of prime Numbers, taken as the Diviſors of Unity. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

Introductory letter.

IN the fourth volume of your Journal, page 403, (4to.) you inſerted a curious property reſpecting the quotients reſulting from the diviſion of an unit by all prime numbers, not leſs than the number 7. I ſhall now lay before you two other properties equally curious, ariſing from ſimilar diviſions, and ſhall be much gratified, if in the hands of ſome of your learned readers or correſpondents, this communication ſhould, as I truſt it will, prove really beneficial.

I am, SIR,

Yours, &c.

H. G.

March 22, 1802.

PRIME NUMBERS.

Law of the quotients of unity ÷ by prime numbers.

1. The firſt half of the quotient figures ariſing from the diviſion of an unit by any prime number, not leſs than the number 7*, will be the complements of the other half, to the number 9*.

EXAMPLE

EXAMPLE I.

$$1 \div 7 = \cdot\dot{1}4285\dot{7}$$

or

$$1 \div 7 = \left\{ \begin{array}{ll} \cdot\dot{1}\ 4\ 2 \ \ . \ \ . \ \ \text{1ft.} \\ 8\ 5\ \dot{7} \ \ . \ \ . \ \ \text{2d.} \end{array} \right\} \text{half.}$$

$$\text{Quotient figures added} = 9\ 9\ 9$$

EXAMPLE II.

$$1 \div 29 = \cdot\dot{0}34482758620689655172413793\dot{1}$$

or

$$1 \div 29 = \left\{ \begin{array}{ll} \cdot\dot{0}3448275862068 \ \ . \ \ \text{1ft.} \\ 9655172413793\dot{1} \ \ . \ \ \text{2d.} \end{array} \right\} \text{half.}$$

$$\text{Quotient figures added} \quad 99999999999999$$

Remark.—From hence it appears, that if half fuch like quotients be found by divifion, the other moiety may be obtained by a fimple addition only.

2. The firft half of the dividend figures (or fucceffive remainders) arifing from the divifion of an unit by any prime number, not lefs than the number 7, will be the complements of the other half to the prime divifor. **Law of the remainders of unity $\div$ by prime numbers.**

EXAMPLE III.

The prime divifor being 17·.

Then $1 \div 17 = \cdot\dot{0}58823529411764\dot{7}$

or

$$1 \div 17 = \left\{ \begin{array}{cccccccc l} 1 & 10 & 15 & 14 & 4 & 6 & 9 & 5 & \text{1ft. half of div.} \\ \dot{0} & 5 & 8 & 8 & 2 & 3 & 5 & 2 & \text{1ft. half of quots.} \\ 16 & 7 & 2 & 3 & 13 & 11 & 3 & 12 & \text{2d. half of div.} \\ 9 & 4 & 1 & 1 & 7 & 6 & 4 & \dot{7} & \end{array} \right.$$

$$\text{Divs. added} = 17\ \ 17\ \ 17\ \ 17\ \ 17\ \ 17\ \ 17\ \ 17$$

$$\text{Quots. added} = 9\ \ 9\ \ 9\ \ 9\ \ 9\ \ 9\ \ 9\ \ 9$$

EXAMPLE

Example IV.

The prime divisor being 23·.

Then $1 \div 23 = \cdot\dot{0}43478260869565217391\dot{3}\cdot$

or

$1 \div 23\cdot =$

1	10	8	11	18	19	6	14	2	20	16	divs.
$\cdot\dot{0}$	4	3	4	7	8	2	6	0	8	6	quots.
22	13	15	12	5	4	17	9	21	3	7	divs.
9	5	6	5	2	1	7	3	9	1	$\dot{3}$	quots.
23	23	23	23	23	23	23	23	23	23	23	Divs. added
9	9	9	9	9	9	9	9	9	9	9	

Remark.—From hence it appears, if half the dividends be found (and they are easily obtained by the continual addition of a few of the first quotient figures) the remainder regularly succeed them as complements of the former.

Observation.—The result of the divisions of an unit by the prime numbers 11, 31, 37, 41, &c. appears at first sight to deviate from the general law above laid down, but in fact it is subservient to it, as will appear from the following

Example.

The prime divisor being 41·.

1	10	18	16	37	dividends.
$\cdot\dot{0}$	2	4	3	$\dot{9}$	quotients.
40	31	23	25	4	dividends.
$\cdot\dot{9}$	7	5	6	$\dot{0}$	quotients.
2	20	36	32	33	dividends.
$\cdot\dot{0}$	4	8	7	$\dot{8}$	quotients.
39	21	5	9	8	dividends.
$\cdot\dot{9}$	5	1	2	$\dot{1}$	quotients.
3	30	13	7	29	dividends.
$\cdot\dot{0}$	7	3	1	$\dot{7}$	quotients.
38	11	28	34	12	dividends.
$\cdot\dot{9}$	2	6	8	$\dot{2}$	quotients.
6	19	26	14	17	dividends.
$\cdot\dot{1}$	4	6	3	$\dot{4}$	quotients.
35	22	15	27	24	dividends.
$\cdot\dot{8}$	5	3	6	$\dot{5}$	quotients.

A table of the quotients and dividends arising from all prime numbers under 1000· is nearly completed, and will be at the service of the public whenever it shall appear sufficiently beneficial.

SCIENTIFIC

SCIENTIFIC NEWS.

Extract of a Letter from Mr. W. WALKER, *Lecturer on the Eidouranion, on the new Planet Ceres.*

To Mr. NICHOLSON.

42, *Conduit-ſtreet, Hanover-ſquare,*
SIR, *London, 25th March,* 1802.

THE intereſt that I find excited, both in public and in private, by the diſcovery of a new planet, induces me to offer you the lateſt obſervations by which it can be aſcertained, that the neceſſary publication of your moſt valuable Journal will admit.

New planet *Ceres*.

Since the 15th of this inſtant March, I have regularly obſerved it; and by the following memorandums it will be readily found by any of your readers, who are ſolicitous, and provided with a night teleſcope, or a common ſpy-glaſs, for it is not viſible to my unaſſiſted eye.

Obſervations on the poſition of the neighbouring ſtars, by means of which the planet may be eaſily found.

If at 9. 10. 11. 12. 1. 2. or 3. in the night, a line is drawn from Theta Lionis, through Beta or the Lion's Tail: at the ſame diſtance, and to the left, but a little above this termination, a ſmall configuration of ſtars will be perceived of the following deſcription: Two ſtars of conſiderable brightneſs will be found to form one ſide of an equilateral triangle, and a very ſmall ſtar the other point of this figure. Between theſe larger ones and the point will be ſeen two minuter ſtars, which, with the ſtar at the point, form alſo an equilateral triangle. The Ceres is paſſing through this cluſter. On the 15th inſtant the planet was to the eaſt of the ſmalleſt ſtar at the point of the triangle. I have regularly obſerved it on the 16th, 20th, 21ſt, 22d, and this evening the 25th, when it is arrived between the two weſternmoſt or largeſt ſtars of the above configuration.

By continuing this line it may eaſily be diſcovered for a few nights to come. The planet is as bright as moſt of the ſtars in its neighbourhood. But although I have obſerved it with magnifying powers from forty to above one thouſand times, I muſt heſitate in declaring it to have a defined diameter

ter or difc, different from the neighbouring ftars; whilft the Georgium Sidus in its neighbourhood leaves no doubt of a decifive magnitude.

I am, SIR, Your's, &c.

W. WALKER.

The following is the right afcenfion and declination of the planet (Ceres), by Mr. Zach of Gotha, for the commencement of the next month *.

	Right Afcenfion in Time.	Right Afcenfion in Degrees.	Declination North.
April 3	12 0′ 12″	180° 3′	18° 6′
6	11 57 54	179 29	18 9½
9	— 55 45	178 56	18 10
12	— 53 46	178 26	18 9
15	— 51 37	177 59	18 5
18	— 50 20	177 35	17 59

Extract of a Letter from M. ZACH, *Director of the Obfervatory of Gotha, to* C. MECHAIN, *of the Inftitute, Adminiftrator of the Obfervatory at Paris.*

Difc of the planet *Ceres*. Sufpicion of two fatellites. Denfe atmofphere.

M. Schroeter, at Lilienthal, obferves with his great telefcope the difc of the new planet Ceres to be about two feconds. He alfo fufpects the exiftence of two fatellites. The planet is enveloped in a very thick atmofphere, for it appears to be furrounded with much nebulofity. I am very curious to know what Dr. Herfchel will tell us concerning it. In the mean time, I thought it proper to write this in hafte, &c. &c.

Elements.

Elements of the new planet, corrected by M. Gaufs, from the lateft obfervations:—

Diurnal heliocentric tropical motion . . . . . 770″ 7376
Tropical revolution . . . . . 1681 days 12 hours 9 fec.

Decade Philof. No. 15. *An.* x.

* A fmall map of the ftars above defcribed was given in Mr. Walker's letter; but time did not admit of engraving it. W. N.

Spider's

Spider's Webs to form the cross Wires in the Eye Piece of Astronomical and other Instruments.

The smallest silver wire I have heard of was rather thinner than one thousandth part of an inch; but every specimen I have seen was considerably thicker. This has not been supposed of much consequence in the construction of plumb lines; but it is very desirable that the wires in the focus of microscopes should be as fine as possible. In a description of instruments in the Cabinet of the Grand Duke of Tuscany, arranged by the Abbé Fontana, and written by Ami Angand in one of the Journals de Physique above twenty years ago, to which I must refer the reader to the index of that useful work, not having a whole sett at hand; mention is made of spider's webs in the focus of microscopes for measuring; and long since, when upon observing that the single thread of the silk worm is very slender, I mentioned its use to my old friend J. H. de Magellan, F. R. S. he informed me, that organized substances had been tried in astronomical telescopes, and rejected, because the solar image was found to burn them.

Smallest wire.

Spider's webs used in microscopes.

Silk worms threads.

I suppose this must have been taken for granted by himself or somebody else without trial; for Mr. Edward Troughton, whose talents are indefatigably directed to every thing that can insure the precision and accuracy of astronomical instruments, has for some time applied the webs of young spiders to the purpose above-mentioned. He takes hold of a short length of the web by means of a forked stick, and then carefully applies it to the marks on the frame or ring intended to hold it, where it adheres by the previous application of a little varnish. These lines have a beautiful aspect for their delicacy, precision, and evenness.

Troughton's application of spider's webs to astronomical telescopes.

That the solar focus should not injure them is a fact of some remark; which I should be disposed to ascribe to their transparency, if I had not been assured by an eminent philosopher, that fine metallic wires were not affected by Parker's great lens, which instantaneously fused and burned thicker wires and masses. What may be the theory of this? Will the ascending current of air cool a small wire more speedily than a larger, of which the internal parts bear a greater proportion to the surface? or does the extreme light ignite and perforate the

Curious fact that the burning lens does not destroy small wire.

Conjectures.

fmall wire, fo as to efcape with rapidity on the oppofite furface, and keep down the maximum of temperature? &c.
Opera defiderantur, non verba. W. N.

Thick Iron burned in Oxigen.

Combuftion of iron in oxigen.

Mr. Accum informs me, that a thick piece of iron or fteel, fuch as a file, may be burned in oxigen gas, if it be made very fharp pointed, and a fmall piece of wood be ftuck upon its extremity, which is to be fet on fire previous to immerfion into the gas.

Extreme accuracy of tuning Mufical Inftruments.

Very exact procefs for tuning mufical inftruments.

About a year ago —— Huddleftone, Efq. fhewed me an excellent monochord, with improvements for afcertaining the precife lengths, and fecuring the equable tenfion of the ftring, which I fhould be glad if I had an opportunity of communicating. I was much gratified by his method of tuning unifons to a degree of precifion which the direct power of the ear could affuredly never command. 1. He tunes his ftring (*a*) (to the tone given by a fork, or otherwife) fo as to have no beat or undulation, but to be as nearly unifon as the fenfe can diftinguifh; 2. he tunes another ftring (*b*) a very fmall quantity fharper (or flatter) fo as to afford an evident beat; 3. he fets a fmall pendulum, or bullet and thread, to vibrate with the beat; 4. he tries his fork with the laft ftring (*b*) and obferves the beat by the pendulum, unaltered; 5. if the beat be flower than before, it follows that the firft ftring (*a*) is ftill too flat, becaufe the fork is nearer unifon with the fharp ftring (*b*);— if quicker, then (*a*) is too fharp. I heard four forks ftruck in quick fucceffion, which had been tuned by this procefs; and though there was only the fundamental, with its third, fifth, and octavo, the effect was delightful.

Strange influence of organ pipes upon each other.

The fame gentleman mentioned a fingular, and I believe unobferved fact, relative to the combination of mufical undulations in the air. Two contiguous organ pipes, both excellent, ftopped diapafan C and C fharp, in a valuable inftrument he poffeffes, govern each other, fo that if C be firft founded and held, the C fharp will give unifon to C whenever its key is ftruck: but if C fharp alone be founded and held, the C pipe will give C fharp when its key is ftruck. I will not venture to fpeculate upon this.

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Printed by W. Stratford, Crown-Court, Temple-Bar.

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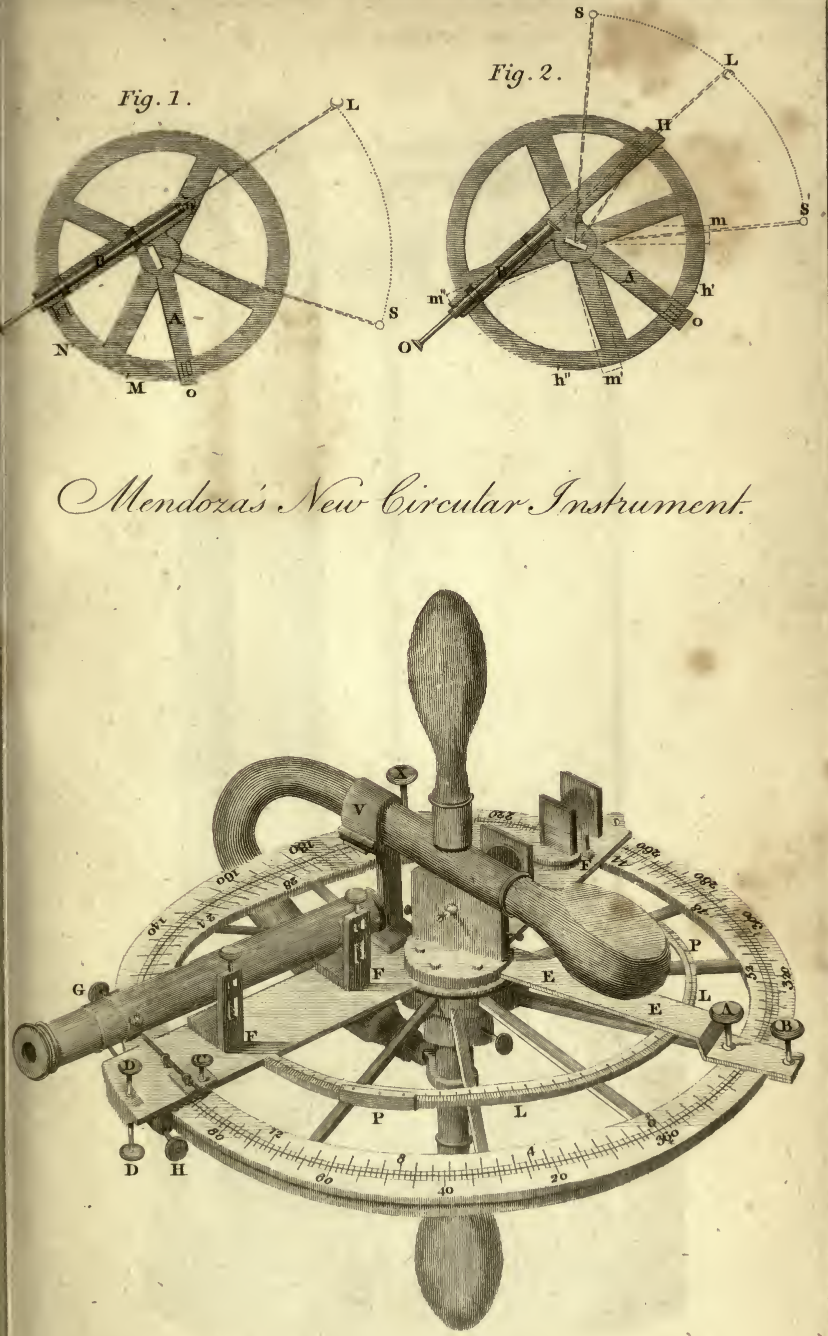
Mutlow Sc. Russell Co.

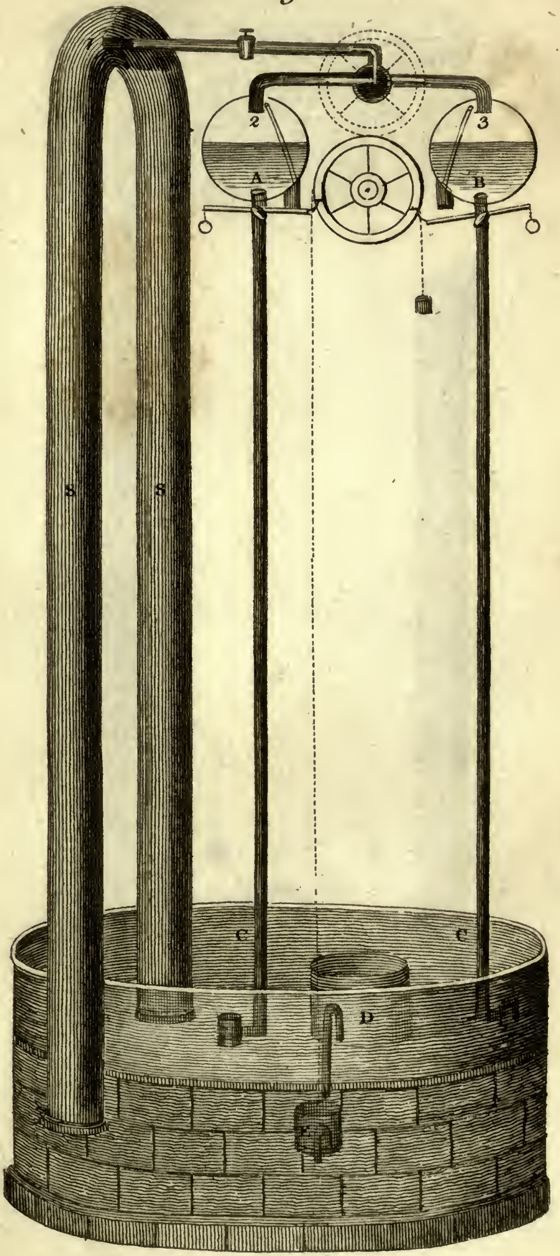


Mendoza's New Circular Instrument.

Mutlow So. Russell Co.t

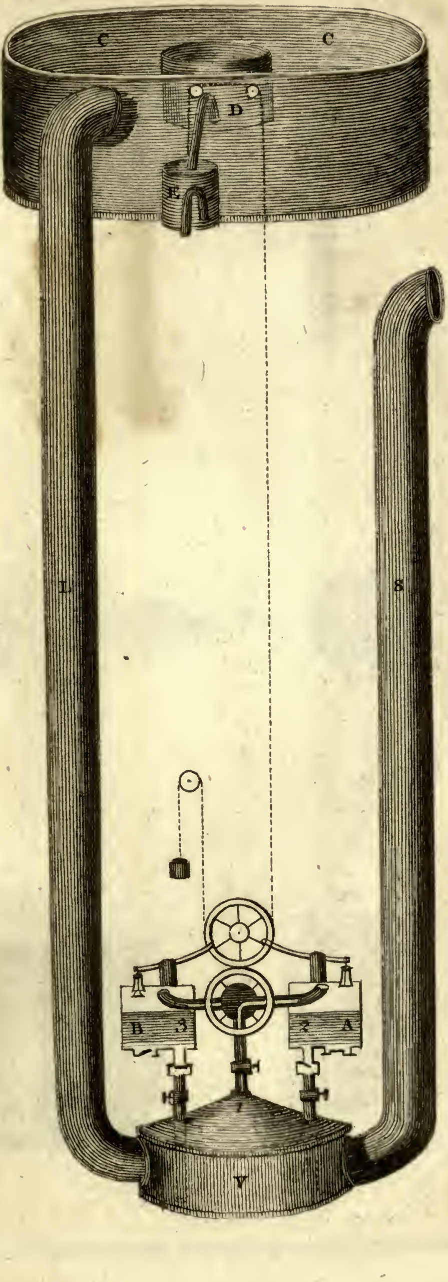
Close's hydraulic apparatus.

Fig. 1.



Close's hydraulic apparatus.

Fig. 2.



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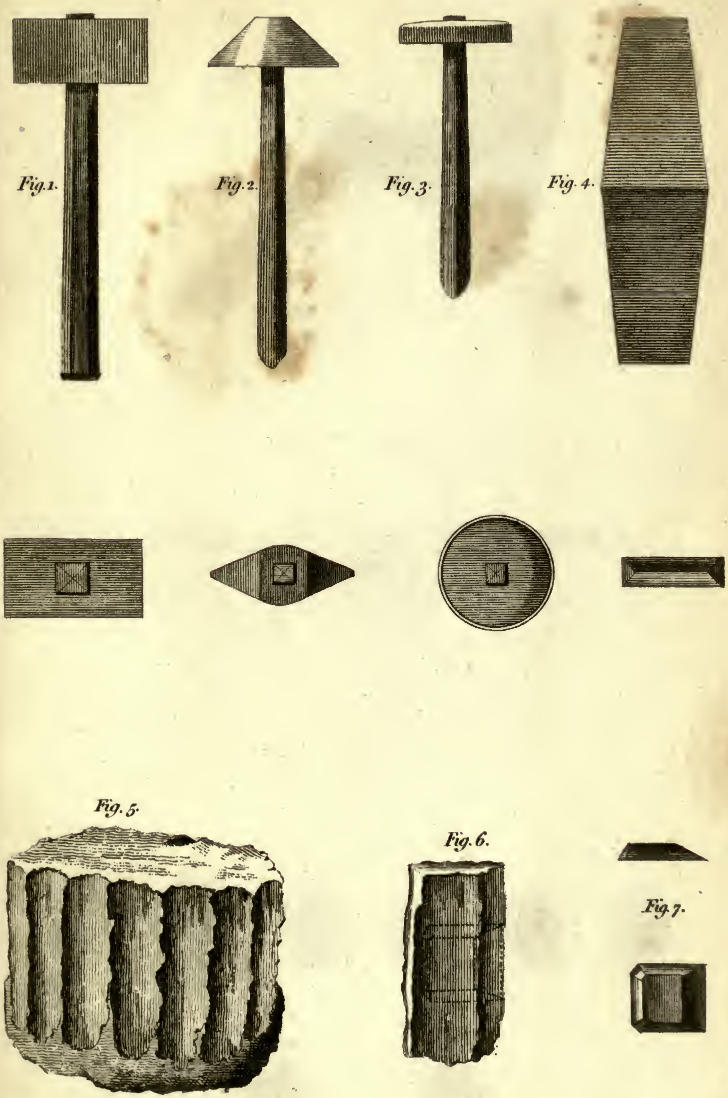
Manufacture of Gun flints.



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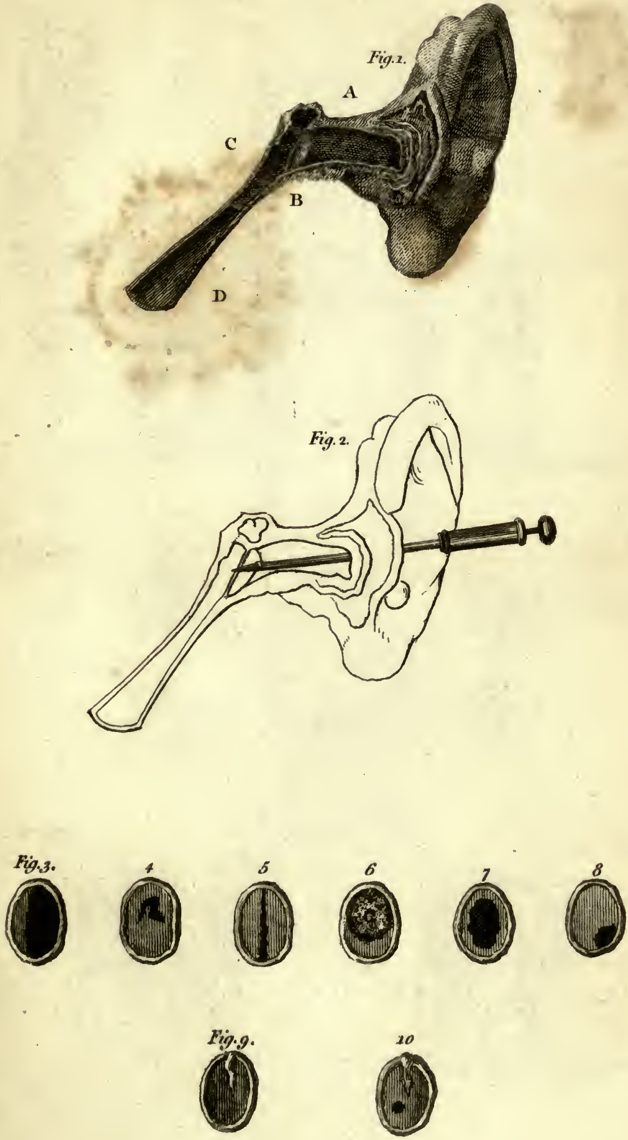
Cure of Deafness by puncture of the Memb: tympani.



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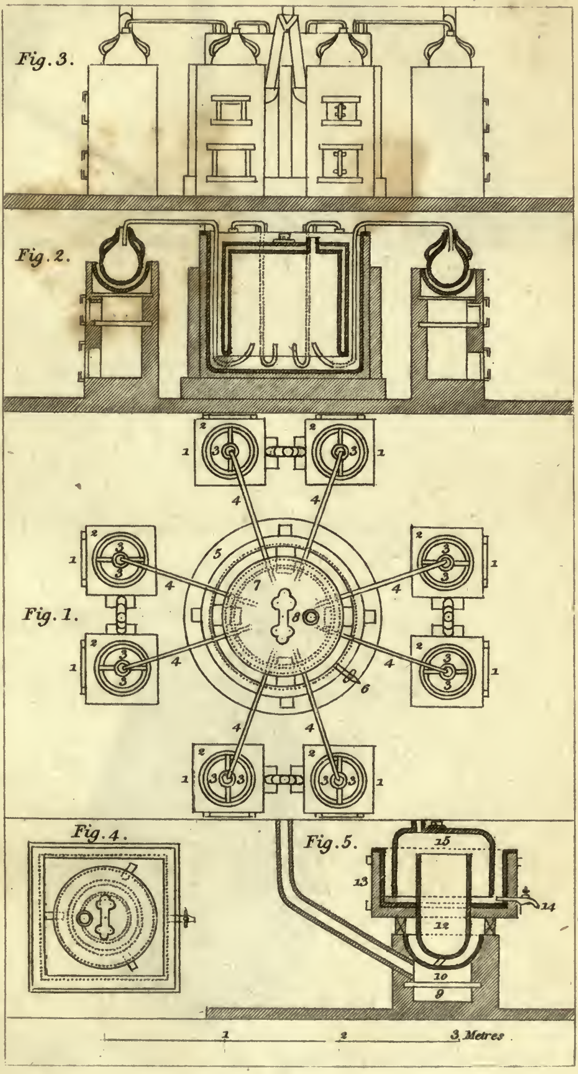
Bleaching of Paper Stuff.



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The Graphometer of Carangeau.

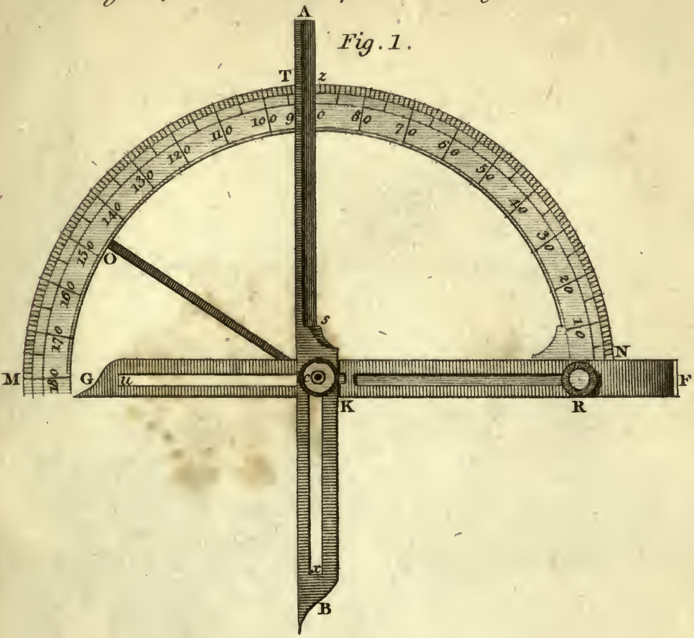


Fig. 1.

Dr. Thompson's Experiments

To shew that Fluids are not thrown into Currents by heat.

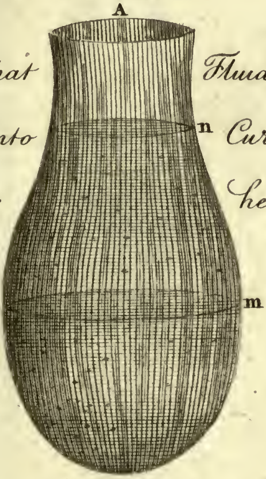
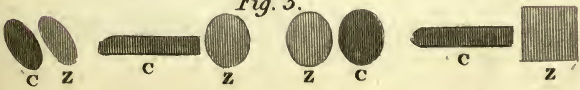


Fig. 2.

Volta's Theory of Galvanism.

Fig. 3.



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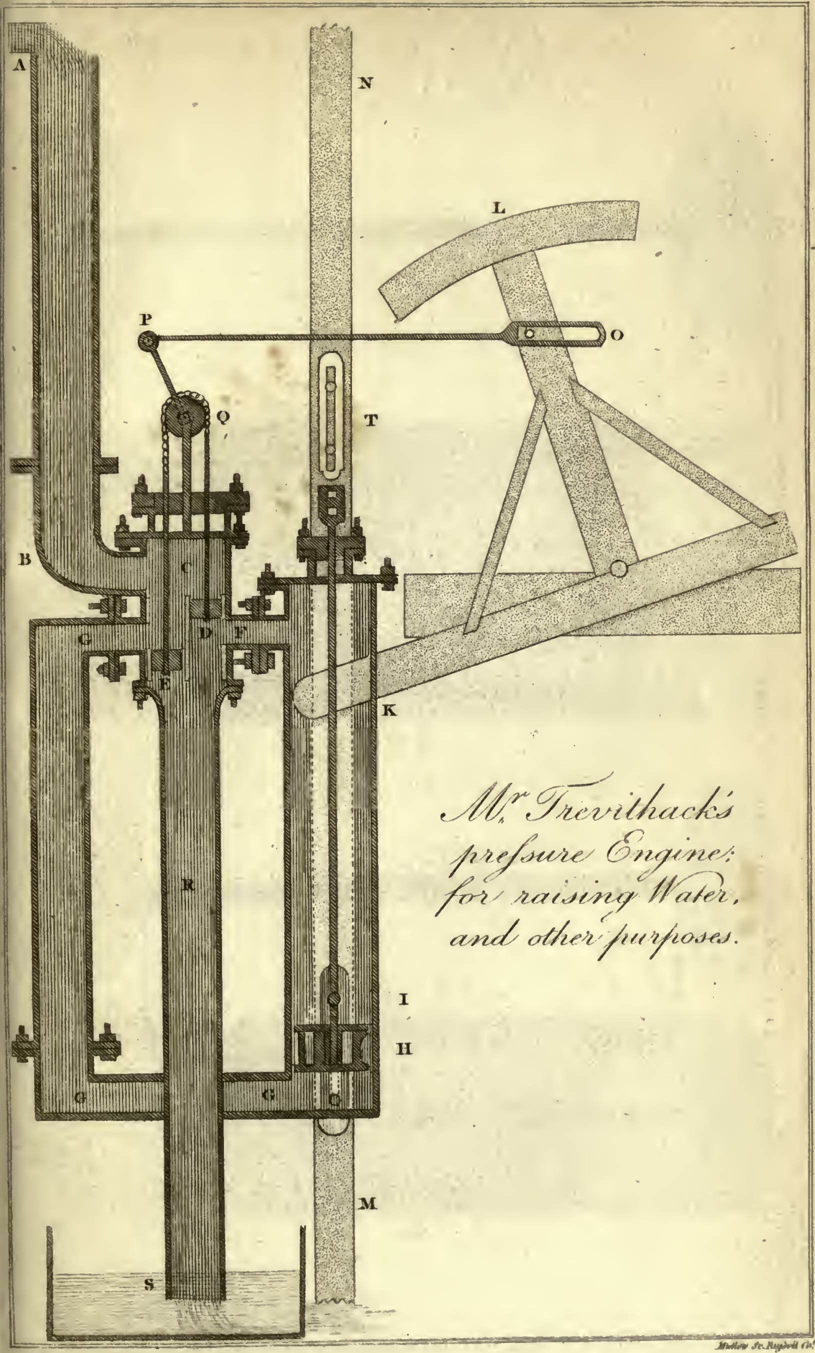

Mr. Trevithack's pressure Engine; for raising Water, and other purposes.

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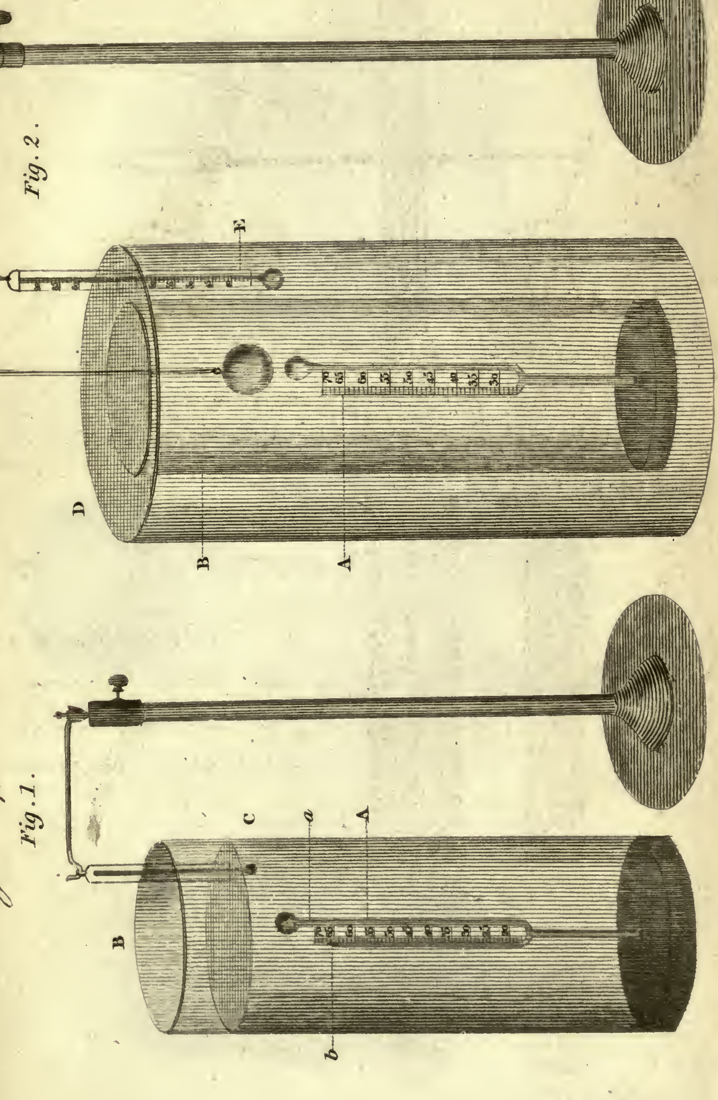
Dr. Murray's Experiments on heat.



Liquids substituted instead of dark Glasses by Herschel.

Fig. 1.

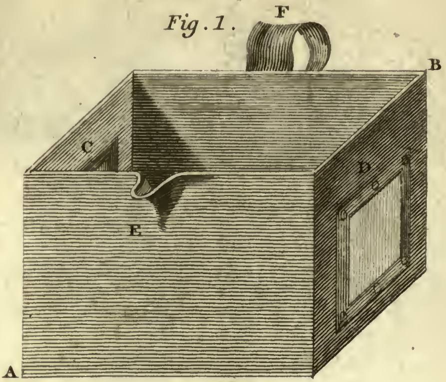


Fig. 2.

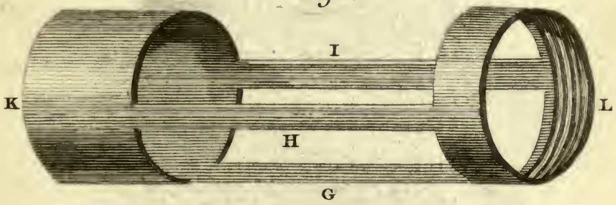
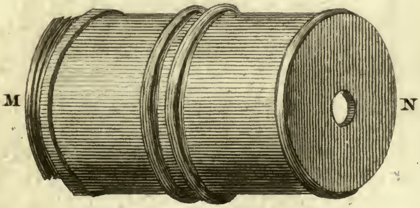
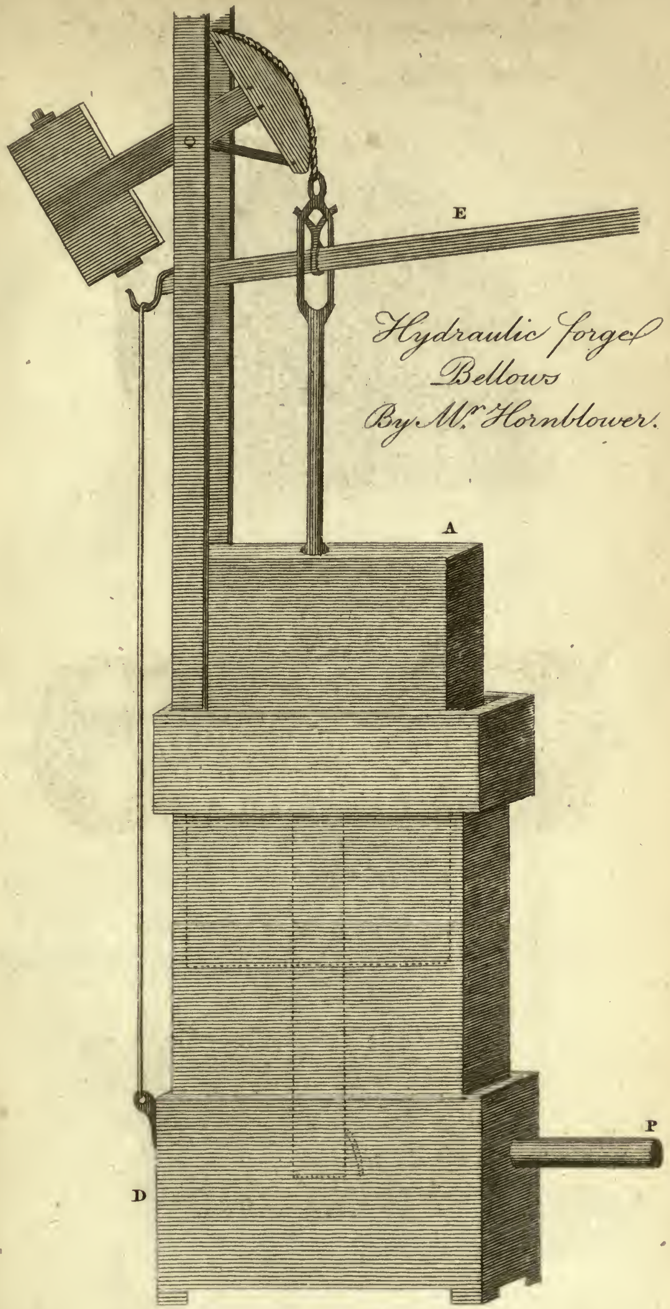


Fig. 3.





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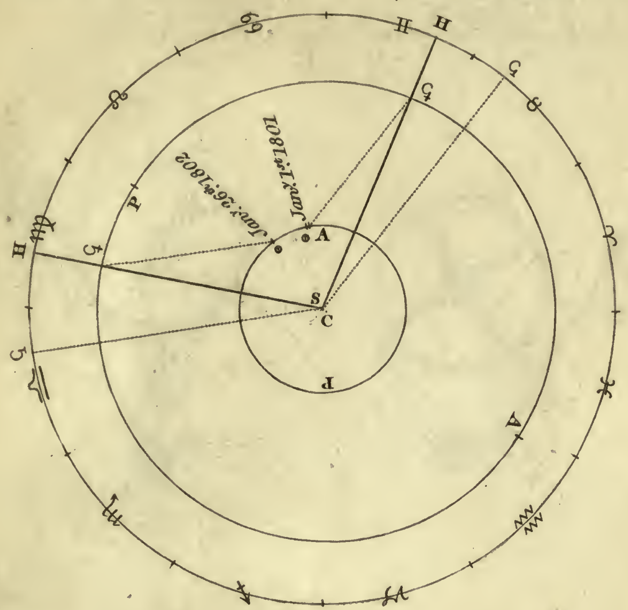
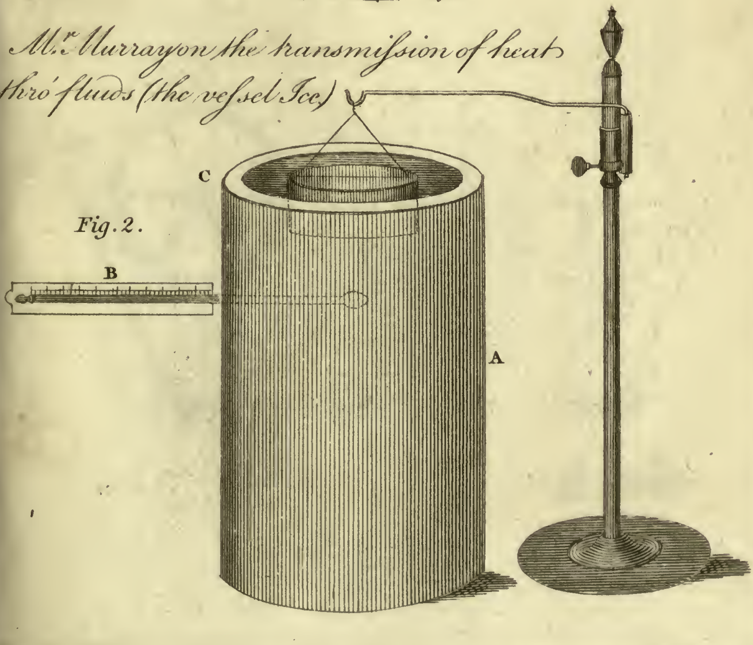
Revd. Mr. Pearson's account of the Planet Ceres



Mr. Murray on the transmission of heat thro' fluids (the vessel Ice)

Fig. 2.



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Mr. I. Delafons Watch Escapement

Fig. 5.

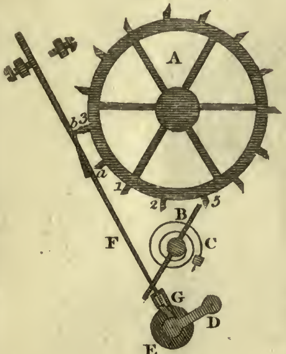


Fig. 3.

Fig. 1.

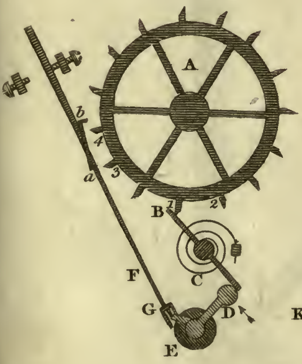


Fig. 2.

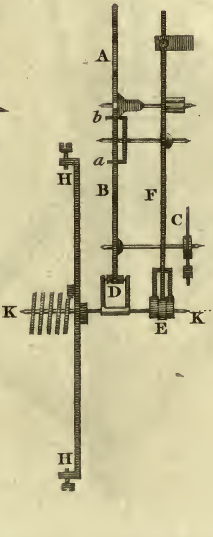
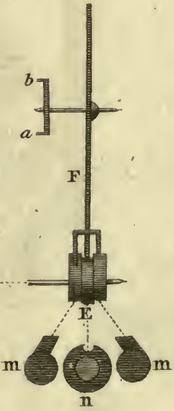


Fig. 4.



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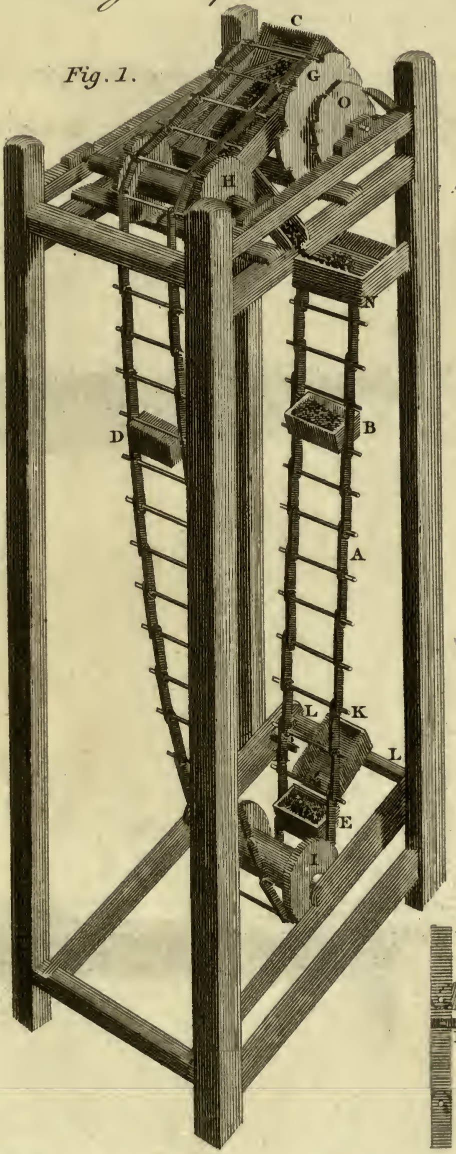
Mr. T. Arkwright's Machine for raising Ore from Mines.

Fig. 1.



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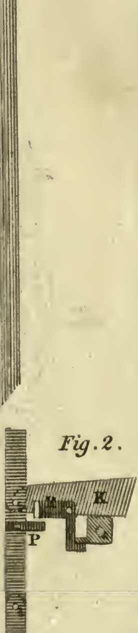

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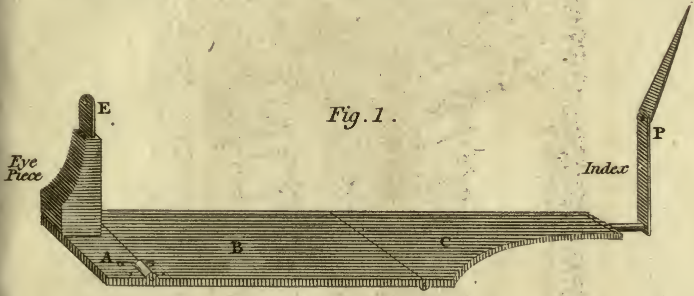
Machine for determining the positions of objects in drawing from Nature.

Fig. 1.



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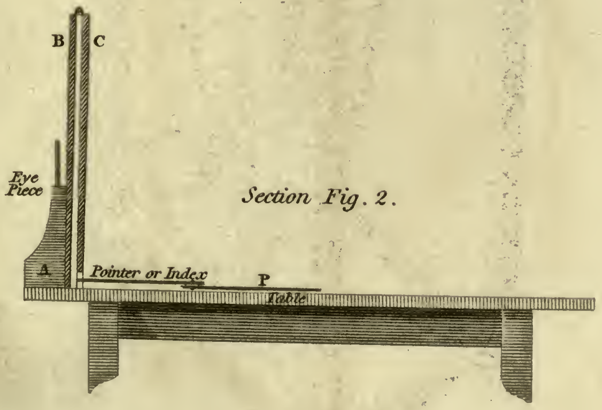

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